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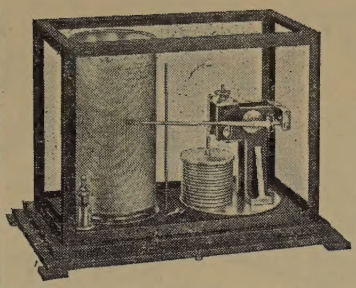

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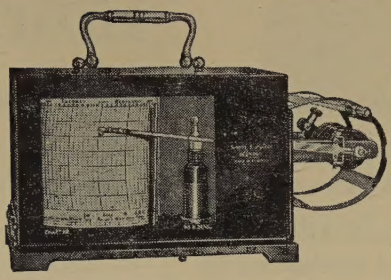

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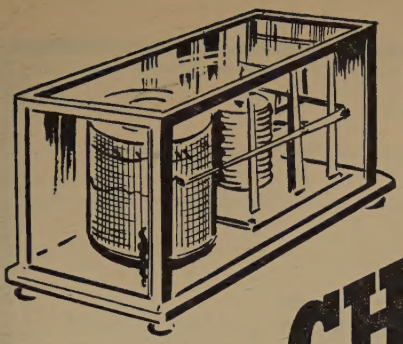

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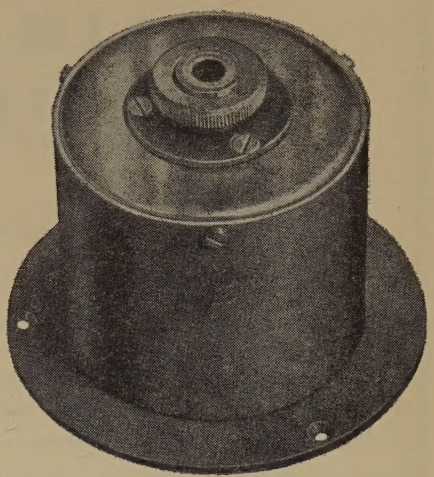

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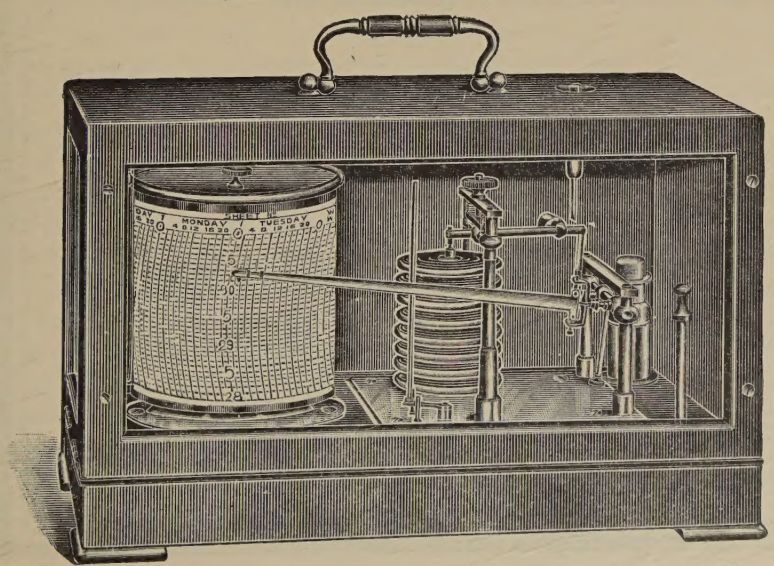

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Water balance and the general circulation of the atmosphere

By R. C. SUTCLIFFE, O.B.E., Ph.D.

Presidential Address delivered before the Royal Meteorological Society on 25 April 1956

1. Introduction

A remarkable feature of world society of the present decade, even of the last two or three years, is the sudden almost feverish awareness of the fact that we live in a scientific or technological age carried along, whether we like it or not, by our own inventiveness. And, while this change is the outcome of science, it is a process with feed-back upon science itself, which has become self-conscious of its virtues : discovery, originality, novelty; research is greater than scholarship. The ancient science of meteorology moves with the stream and refreshing novelty emerges everywhere, offering a wide choice to a President who wishes to strike a modern note. But no change, however desirable, is wholly good and I have thought that the vogue for originality has diverted too great a proportion of the efforts of able workers away from the major central problems of our science to attractive innovation. New and interesting branches are being worked assiduously; may one with temerity mention radar and radio-meteorology, radioactivity, atmospheric chemistry and atmospheric ozone as attractive subjects guilty of enticement ? And, considering how limited is the activity of British universities in meteorological research of any kind, it is quite remarkable to see the amount of interest taken in the very high atmosphere. Looking around the country I have formed the fanciful picture of a great garden of meteorology with the main area of weather and climate tilled for a living by hard-working hands, while gentlemen of leisure tend exotic plants in neighbouring cloisters.

This picture is a libellous caricature but with sufficient basis to encourage me to take as my subject something very old which lies in the main body of our science. If I should pick out from meteorology one problem of pre-eminent importance which languishes through neglect, my choice would be the general circulation of the atmosphere. I do not really think it is necessary to define what this expression means, but, for the guidance of some of my friends who occasionally confess or claim ignorance, I may remark that the problems of meteorology are set by the specific phenomena and modes of behaviour which characterize the atmosphere, and on many scales in time and space. Amongst these there is none more specific, more characteristic, or more clearly defined than the world-scale pattern of flow when averaged over long periods of time : the problem is to determine the structure and to understand, in terms as fundamental as possible, how it comes about. Nothing quite like it is known to exist anywhere else in the universe and so far there is no basic theory to hand.

My aim, in this short address, is to be frankly discursive and suggestive rather than original or profound but, even so, the whole subject is too broad for my purposes and I have taken the circulation of water in the atmosphere as my theme. By dealing briefly with evaporation, precipitation and transport in the atmosphere I hope to indicate the nature of the problem, and shall start with precipitation.

2. Precipitation

It is part of my object to stress the importance of detailed mechanism in the wider problem and I propose here to survey in this context the mechanism of precipitation, referring both to the microphysics and the macrophysics. Perhaps because the point is too obvious it seems never to be noted that the microphysical properties of cloud and precipitation are basic to the general circulation.

A critical and much studied stage in the precipitation mechanism is the transition from clouds with particles having almost negligible fall speeds to raindrops or snowflakes. We know of two effective processes, coalescence between particles and that arising from the occurrence of ice particles within a supercooled-water cloud. We also know that neither is effective in many clouds which form and disperse with no precipitation, and the efficiency of precipitation release of natural clouds so becomes a basic quantity in meteorological theory. It is at least conceivable that no precipitation mechanism should be available, in which case clouds would remain continuously as airborne aerosols, to disperse only by evaporation. In such a case, since evaporation from the surface must also cease, the only possibility would be an atmosphere of varying cloud density virtually saturated everywhere at the earth's surface. In dynamical calculations of cyclonic rain rough accuracy is obtained by assuming 100 per cent precipitation efficiency (all condensed moisture is precipitated), but however good the approximation may be in large-scale dynamics it would be highly erroneous in a fundamental thermodynamical theory of the general circulation. The albedo of the earth, so important in the energy balance, is determined mainly by the non-precipitating clouds which account for some 90 per cent of total cloudiness. It is sometimes said that interference with clouds by artificial seeding could only redistribute rainfall and could not augment the world total; it remains to be shown that artificial seeding can do anything at all of practical significance, but if precipitation efficiency becomes subject to control not only local rainfall but the general circulation is vulnerable.

To pass to the macrodynamical factors we recall the convenient classification of rainfall as orographic, convective or cyclonic, while accepting, of course, interactions and complications. Orographic rainfall has an intensity, scale and distribution determined largely by the orography itself, and its importance in the general circulation of moisture is climatologically obvious. But it is equally significant that convective and cyclonic rains are also highly specialized ' macro-meteors ' with marked geographical distributions. In the familiar idealized general circulation model the typical disturbance of the equatorial rain-belt is the convective cell of horizontal dimensions 1–10 km; in the middle-latitude rain-belt the typical disturbance is the cyclone of horizontal dimensions 1,000 km.

There is no doubt that the system of vertical convection is an instability phenomenon driven by condensation; the importance of water in the dynamics of extratropical cyclones is less well established, but my own view, based on the significance of vertical stability in the dynamics of development as well as on energy considerations, is that it is vital. The point I wish to make here is that the nature of the large-scale turbulence structure of the atmosphere is highly characteristic, it depends on the dynamics of preferred instabilities and is different in different latitudes.

In middle and high latitudes long-term balance of properties is largely brought about by the eddy motion on the cyclonic scale and there is, as yet, no certainty about mean circulations in the meridional plane. In equatorial latitudes horizontal eddy transport is less important and meridional overturning is a dominant feature. It seems to me that this is the case almost entirely because in the one region very large horizontal eddies do occur while in the other they do not : were the turbulence structure different the whole general circulation pattern would be different also.

Further to make this point, I draw attention to another type of water-driven turbulence element, a sort of hybrid between the convective and the cyclonic system – the tropical cyclone. In accounts of the general circulation it is usually ignored or added as an embroidery of no basic importance except for those areas subject to its violence. But as a climatic element it is perhaps more critically balanced than any other macrometeor and it would, we may conclude, need little change of conditions to render it either a non-existent feature or a regular feature of all tropical oceans and all seasons, an important agency of the general circulation.

On this reasonable argument we are justified in concluding that our classical model of easterly trade winds converging to the equator is by no means sacrosanct. With not greatly different external conditions we may expect a solution with mean surface westerlies and relatively high pressure at the equator, bounded in each hemisphere by a tropical cyclonic belt. Indeed, in the central Pacific there are symptoms of this mean pattern with a weak minimum of precipitation near the equator. The repercussions of such an equatorial circulation on the general circulation elsewhere cannot be judged without a quantitative theory, but if the occurrence of tropical cyclones is as critical as climatic evidence suggests and the conclusions of Bergeron (1954) and Palmén (1948) support, we have here a possibility for climatic change not yet explored and a delicate factor to handle in any basic theory of the general circulation.

3. Evaporation

The problem of evaporation has given rise to a very considerable literature concerning both the micrometeorology of the boundary layer and the integrated regional totals over both land and sea. The former aspect was taken by our previous President, Sir Graham Sutton, in his Address (1954), as an illustration of meteorology as an exact science and no doubt in a certain sense the problem is well on towards being quantitatively solved. Unfortunately, when it is necessary to estimate evaporation on the synoptic or climatological scale, the data on humidities, winds and temperatures, with their vertical profiles, are not available and are unlikely to be so in the foreseeable future; all the realistic estimates rest to a large extent on empiricism only partly justified by theory.

Before going on to outline the methods which have been used, attention may be drawn to a gap in our knowledge. The mechanisms of precipitation are parts of the established subject matter of synoptic meteorology but the synoptic meteorologist, particularly the weather forecaster, has given negligible attention to evaporation although this is also controlled by the synoptic situations, by the cloudiness, the wind and the air-mass properties. While therefore we have much information on how the distribution of precipitation is related with the synoptic patterns, we have practically no quantitative knowledge on how the distribution of evaporation is so related. The nearest approach has been the regional estimates of evaporation over the land for periods of many days or weeks, using formulae due to Thornthwaite, Penman and others, and here the approach pays no attention to the synoptic patterns as such. Synoptic studies of evaporation might be profitable, and if a moisture balance is to be struck for periods of abnormal synoptic

conditions, such studies will be essential. The major difficulty is that evaporation is entirely unmeasurable over the sea, and over the land evaporimeters are no more than crude indicators; estimates depend on calculations. As I shall not have time to discuss the complex problem of evaporation and evapotranspiration over the land I shall be content to refer to Penman (1956) for a recent survey of the subject.

On the world scale the average or climatological distribution of evaporation over the oceans has been estimated by only a few workers of whom the first were, I believe, Schmidt (1915) and Wüst (1920). In recent years we have three important attempts. Albrecht (1949) produced charts for the Pacific – Indian Ocean area for the months of February and August. Jacobs (1951) gave seasonal maps for the northern oceanic areas and Budyko, Berlyand and Zubenok (1954) described methods of estimating monthly maps which are published in the *Atlas of heat balance* (1955). My present purpose is to indicate how the subject now stands and it will serve to outline Jacobs's procedures.

With some preliminary appeal to diffusion theory, Jacobs assumes that a formula

$$E = K(e_w - e_a)\, W_a$$

should be a reasonable method of estimating the monthly evaporation E from monthly mean values of e_w and e_a, the vapour pressures at the sea surface and in the air at the level of ships' observations, with W_a the mean air-speed. The evaporation factor K remains to be estimated by other methods and an energy balance calculation was carried out for four sea areas chosen as having minimum heat exchange by oceanic advection. The values of K so obtained differed considerably but a mean value adopted, $6{\cdot}0 \times 10^{-6}$, gave a reasonable overall fit.

The method of determining all the other terms in the heat account of the ocean surface is then a vital matter, since for the test areas evaporation is computed as a residual. I do not wish to go into details, for which reference must be made to the original papers, but it is useful to keep the essentials in mind. The short-wave radiation intake is estimated from a formula due to Mosby (1936) relating the incoming radiation with clear sky to mean solar altitude, and allowance is made for cloud albedo according to the degree of cloudiness. The long-wave radiation exchange is estimated from ' surface observations ' of temperature and humidity using a formula due to Ångström (1920) and is again corrected for cloudiness using a factor due to Asklöf (1920). An overall multiplying factor 0·94 is introduced as the oceanic surface is believed not to behave as a perfect black body. The total radiative exchange having been determined in this way, the net balance represents the loss by evaporation and convection and is divided between them according to the Bowen ratio approximately assessed from ships' observations.

Budyko determines oceanic evaporation by the same basic formula which, however, he credits to Shuleykin. The constant K is assigned a smaller value and was estimated from studies of lake evaporation and other evidence.

Although the procedures are entirely reasonable it is clear that they can be of only moderate accuracy. The evaporation determined by energy considerations is a rather small residual and however accurate the fundamental equations may be the substitution of mean values for many quantities in non-linear relationships renders them inaccurate to an unknown degree and little more than plausible and empirical. It is also important to remember that the constant K depends on the data, ships' observations, which are used, and Jacobs draws attention to this point. Privett (in unpublished work) finds that Jacobs's formula when applied to British ships' data gave some 50 per cent greater evaporation and the indication is that the differences in accuracy, methods or times of observation in the two sets of data were important factors. One forms the impression that although there is scope for some further work on these lines the intrinsic difficulties are such that

estimates are not likely to be greatly improved unless and until direct measurements can be made.

4. World distributions

I wish now to refer briefly to the distributions of mean precipitation and mean evaporation over the globe as they have been estimated by various authorities. While the broad patterns are no doubt reasonably well established the actual values are uncertain in many respects, with the reliability of different estimates difficult to assess.

The basic difficulty, as we have previously said, is that precipitation is not measurable with any accuracy over the oceanic areas and evaporation is directly measurable neither over land nor over sea. Thus the estimates are arrived at on the basis of judgments, weighting various pieces of evidence, and authorities do not agree. The position is further confused by the natural tendency for different authors to accept different results previously obtained so that it is difficult to decide exactly at what stage the disagreements arise.

Brooks and Hunt (1930) made original determinations of precipitation for land areas using actual data and their values have been generally accepted by later writers. They were critical of other attempts made earlier, stating that they were nearly all based on Supan's charts of 1898. For oceanic areas they accepted Schott (1926) for the Atlantic, and Süring (Hann-Süring 1926) for Pacific and Indian Oceans. Brooks's values agreed well with those of Kerner (1910, 1919) which Wüst (1922) had regarded as much too high on the basis of evaporation estimates.

Meinardus's (1934) chart of mean annual rainfall is widely quoted but this was essentially from Schott (1926 and 1933) for the oceans and was criticized by Möller (1951) in the same sense as Wüst's criticisms. Möller gives four seasonal maps and having considered evaporation, from energy balance, amends Meinardus's oceanic values by a factor 80 per cent; Wüst estimates 72 per cent, Mosby 84 per cent. There are other maps available which the writer has not examined : these are charts referred to by Jacobs (1951) and Russian charts referred to by Budyko *et al.* (1954).

As regards evaporation the most complete charts yet published are the monthly means in Budyko's *Atlas*, already referred to, and Jacobs's seasonal charts for the northern oceans. Since the world totals of evaporation and precipitation must be equal (there being no storage trend in the atmosphere), it is a matter of judgment whether one puts greater weight on precipitation values as estimated from island stations or on evaporation calculations based on a rather weak chain of energy calculations. For what they are worth, I give in Table 1 some estimates given or implied by various authors for the annual totals over land and over sea. These illustrate the range of opinion and I do not presume to make another pronouncement. Short of starting from the beginning, throwing aside

TABLE 1. Annual totals (in millimetres)

	Precipitation			Evaporation		
	Land	Sea	World	Land	Sea	World
Brooks 1930	659	1,107	975	–	–	975
Mosby 1936	–	–	–	–	1,060	–
Wüst 1936	670	820	780	420	930	780
Möller 1951	670	900	832	420	1,000	832
Reichel 1952	670	875	810	470	955	810
Jacobs 1951	–	812	–	–	–	–
Wüst 1954	–	897	–	–	973	–
Budyko 1954	650	1,050	930	420	1,150	930

all ' authorities ' and reassessing all the evidence – an enormous task – there seems little that is worth attempting.

There appears still be to a range of 20 per cent in oceanic estimates and something near this in world estimates. Unpublished estimates of evaporation by Privett for the southern oceans are 20 per cent greater than those of Wüst (1936), again emphasizing the uncertainty. Since only some 20 per cent of incident solar energy is used in evaporation, an error of 20 per cent is only 4 per cent of the incident energy and some very nice estimating is required to be sure of such accuracy. Wüst (1954) claims an accuracy within 10 per cent for world totals and 15 per cent for 5° latitudinal zones and this is a fair admission of the position, even a little optimistic.

As regards the patterns of evaporation on the broadest scale there is little difference of opinion. In the southern oceans Budyko finds maxima near 15°S largest (exceeding 250 cm/yr) in the central Indian Ocean, over 200 cm in the west Pacific but not attaining 150 cm in the south Atlantic. In the northern hemisphere the maxima are at somewhat higher latitudes and concentrated in the west, near 20°N in the Pacific and extending over the Gulf Stream to near 40°N in the west Atlantic. The equatorial minimum falls below 100 cm in the Atlantic, below 125 cm over much of the equatorial Pacific but fails to appear over the Indian Ocean where totals decrease northwards to a minimum at the coast line. Jacobs agrees fairly well for the northern oceans, but Privett's unpublished charts for the southern oceans are in only the broadest zonal agreement with Budyko and in all cases the detailed features of the isopleths have rather little in common.

Reichel (1952) and Wüst, Brogmus and Noodt (1954) give interesting diagrams of the latitudinal distributions of total zonal precipitation and evaporation, and Fig. 1 is derived from Reichel. This brings out the striking difference between land and sea. The maxima over the land are in the lowest latitudes but over the sea, which is indicative of what we should expect to find over a symmetrical oceanic earth, we have the three precipitation maxima, in equatorial and in middle latitudes, with two evaporation maxima in the trade wind zones, out of phase with the precipitation maxima. Jacobs (1950) gives a special study of the distribution of the difference between evaporation and precipitation over the North Atlantic and North Pacific. The implications are important in connection with vapour and latent energy transport, which is now considered.

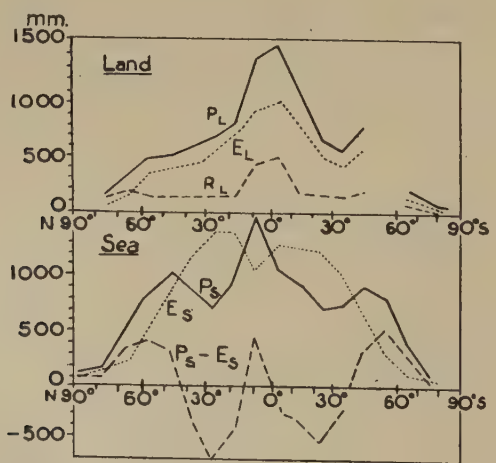

Figure 1. Precipitation, evaporation and run-off for latitudinal zones in mm/yr, separated for land surfaces and oceans. After Reichel (1952).

5. Vapour transport

Studies of vapour transport on the world scale have been mainly concerned with meridional flux and general circulation balance as between different latitudinal zones. The most complete study is that of Starr and White (1954) based on a network of some 75 aerological stations in rough latitudinal bands near 13°, 31°, 42·5°, 55° and 70°N (although the data for the lowest latitude are missing for the vapour estimates). The following table gives their estimates of northward flux of vapour compared with balance requirements as estimated by Wüst and others. The total eddy flux is of the same magnitude as the

TABLE 2. After Starr and White (1954). Northward flux of vapour $\times 10^{11}$ g sec^{-1}

Lat °N	Balance requirement Wüst-Conrad 1936	Wüst-Benton 1950	Horizontal eddy flux	Adjusted total
70	0·8	0·5	0·9	0·8
55	3·6	3·2	3·4	3·3
42·5	7·5	5·5	3·8	6·5
31	6·2	3·3	5·2	5·1

required transport, providing good confirmation of the accepted view that meridional fluxes in these latitudes are largely the result of the unsteady motions. The synoptic meteorologist who habitually thinks in terms of moist air masses coming from low latitudes and dry air masses from high latitudes will hardly need to be persuaded of the reality of this result. In low latitudes however the position seems to be quite different. Riehl, Yeh, Malkus and La Seur (1951) have studied the moisture economy of the trade winds and Palmén (1955) has shown that the precipitation-evaporation excess of the equatorial belt may be fully accounted for by steady transport in the Trades. Indeed, in these latitudes, eddy flux is likely to be down the gradient of moisture content and so opposed to the net flux which is towards the moister region.

These studies are of decisive importance in the building up of a firm structure for a general circulation model but they do not go far enough if we are concerned with regional moisture balance on the earth, with its great asymmetries of evaporation and precipitation. Little has been done in this direction but we have a notable paper by Benton and Estoque (1954) for the North American region in which they perform a computation of aerological moisture balance on the basis of daily upper air data. In this context it is interesting to recall that hydrologists have made great use of the water-balance equation for the land surface, which may be expressed as

$$P = E + R_l + \Delta S_l$$

showing that P, the precipitation, is lost partly in evaporation, E, partly in run-off from the land, R_l, and the remainder, positive or negative, is the increment in land storage, ΔS_l. There is the corresponding aerological water balance equation

$$P = E + R_a + \Delta S_a$$

expressing the fact that, viewing the atmosphere as the reservoir, its loss of moisture by precipitation is made good partly by evaporation, partly by run-on (or advective convergence) in the air, R_a, and the balance is a loss or gain in storage in the atmosphere above the region in question. This latter quantity, ΔS_a is determinable with quite sufficient

accuracy from a reasonable network of aerological stations while the advective convergence may be estimated from integrals of the moisture advection across the boundaries of the region. Thus, with precipitation measured, evaporation can be deduced from purely aerological data. There are practical difficulties due to inadequacies of data but provided the area is reasonably large the method should be a powerful one for regional studies. Benton and Estoque conclude from their studies that certain estimates of evaporation made from hydrological considerations have been considerably in error and one would like to see their technique applied much more widely. For stations which combine wind with humidity observations it would be a simple routine calculation to compute the vector quantity, moisture transport, and if daily values and monthly totals were published it would be possible to study the processes synoptically and climatologically. With energy and momentum fluxes similarly handled the study of the physics and dynamics of circulations would become much more practicable.

A point of some interest is that high moisture transport across an area is not closely correlated with high precipitation. The divergence of moisture gives only the excess of evaporation over precipitation and to a considerable extent a given area lives on its own resources with evaporation and precipitation balancing. The whole atmosphere with the surface below is a closed system and any large geographical part of it may be nearly closed

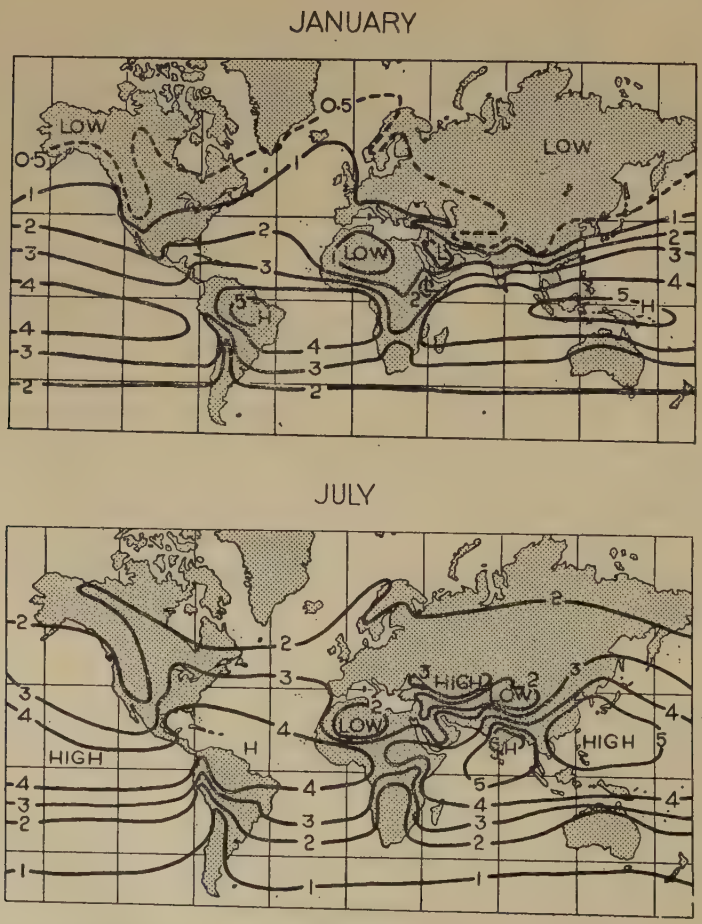

Figure 2. Total mean moisture content of the atmosphere for January and July in centimetres of precipitable water. Prepared from aerological data by J. K. Bannon (unpublished).

in respect of moisture balance : the point is significant in considering rainfall anomalies which may be largely self-maintaining especially over land areas in summer, where the same moisture may be turned over continually or dry ground may indirectly inhibit precipitation.

6. Some general comments

The various aspects of evaporation, transport and precipitation-release of moisture which have been sketched in these remarks lead to a number of conclusions of importance in studies of the general circulation.

First, the total of evaporation or precipitation, some 90 cm/yr, may be compared with the atmospheric storage averaging only 2·5 cm of water equivalent. Mr. J. K. Bannon has recently produced new estimates of the world distribution of moisture in the atmosphere directly from aerological reports, and the two charts of Fig. 2 show part of the results while Table 3 gives figures of average amounts in each hemisphere. In the northern

TABLE 3. Mean vapour content of the atmosphere in centimetres of precipitable water

	Northern hemisphere	Southern hemisphere	World
January	1·9	2·5	2·2
July	3·4	2·0	2·7

summer the whole atmosphere contains about 0·5 cm more moisture than in the northern winter and the oceans should be so much the shallower – although land storage of water and ice is probably a greater term. But as regards the *atmospheric* economy, the point has

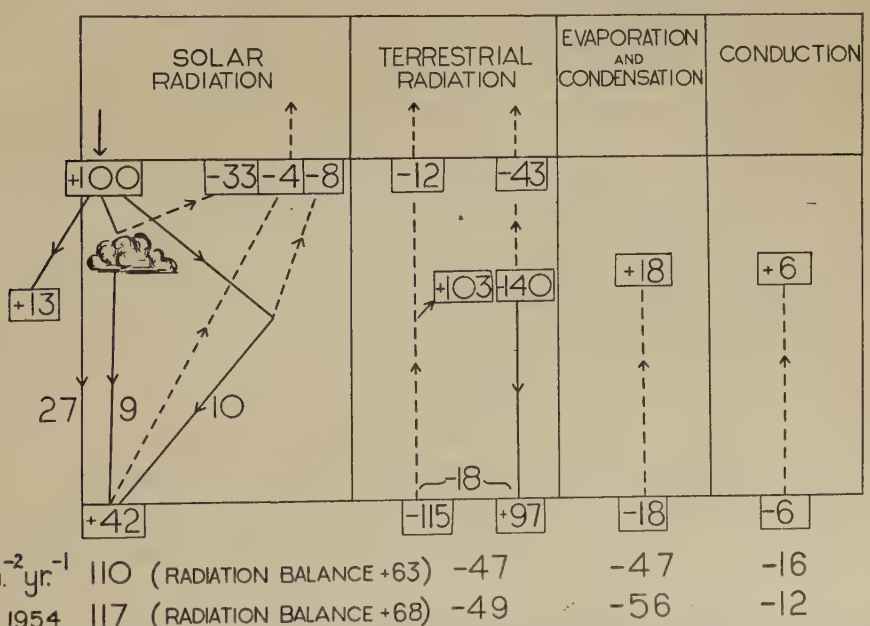

Figure 3. Percentage energy balance for earth and atmosphere after Albrecht (1949). Values in K cal cm^{-2} yr^{-1} are based on a solar constant of 2 cal cm^{-2} min^{-1}. Values derived from Budyko (1954) are given for comparison.

little significance and the more important fact is that the storage is small compared with the overturning : as has frequently been noted, the atmosphere contains only some ten days' supply of average rainfall. It is clear from this consideration that the state of the atmosphere weeks or more ahead will never be calculated by an integration through time requiring moisture or energy gains and losses to be totalled. If long-period forecasting is to be successful it must come from less direct methods.

I would like also to comment on the energy balance and for this purpose the diagram of Fig. 3 after Albrecht (1949) is convenient. Accepting these figures, only 42 per cent of incident solar radiation is absorbed at the surface and only 40 per cent of this is used in evaporation – 18 per cent of the whole. The partition of energy is quite sensitive to cloudiness (owing to the high reflectance of cloud for short-wave radiation and its nearly black-body properties for long-wave radiation) and calculations of evaporation in this way can hardly be within 10 per cent.

In discussing the energetics of the general circulation there is liable to be confusion due to the failure always to distinguish the system under discussion, which is the atmosphere, not the earth as a whole nor the surface below. For the atmosphere the direct energy input and loss have a world pattern rather closely following the pattern of precipitation (see Albrecht 1949) with a comparatively uniform cooling to space providing the overall balance; regional balance is through the air circulation.

Frequently one meets the statement that the circulation, steady or turbulent, is required to adjust an excess of heating at low latitudes and a deficit at high latitudes but this does not seem to me to be helpful. Rather should we say that the turbulent breakdown of the zonal circulation sets up an excess of heating in equatorial and middle latitudes, mainly through condensation, leaving a deficit in sub-tropical and high latitudes, and that the situation is self-balancing through the circulation. Thus the mean circulation cannot be inferred as a consequence of a pre-determined pattern of energy input. In formulating the problem one would rather confidently expect that a turbulence structure expressing the highly specific characteristics of baroclinic instabilities would need to be built into the model or, better, emerge from more basic assumptions, and it is hardly reasonable to hope that theory which omits the water cycle can have much quantitative realism although it might have significant points of similarity with reality.*

Finally I would like to reiterate my opening remark. The problem of the general circulation remains the greatest unsolved problem of meteorology and one would like to see it analysed and attacked in a concerted manner. Its ramifications concern many branches of our science; it is eminently a problem for co-ordinated attack and I would like to see this Society create a precedent in its long history by setting up a group of active workers drawn from different disciplines with the object of formulating methods of attack and pursuing them. If this proposal is well received I am sure there are resources in this country which could be put into the enterprise.

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* Since drafting this address I have had the pleasure of reading papers by Phillips (1956) and by Fleagle (1957) which respectively gained the award and a high commendation in the first competition for the Sir Napier Shaw Memorial Prize of the Society. It is significant that both approach the general circulation problem from the instability of the westerlies with little attention to the tropical circulation.

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The polar aurora

By J. PATON
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Symons Memorial Lecture, delivered 11 April 1956

1. Introduction

Aurora is one of the most fascinating of natural phenomena. It is a privilege to be permitted to choose it as the subject of a Symons Memorial Lecture and I am grateful to the President for the honour he does me in inviting me to give the lecture. Perhaps, living in a place ideally suited for observation, I may claim to have watched aurora during the past 20 years for more hours than any one in our Islands. They have not maybe been rewarding hours in the sense that they have led to any significant contribution by me to our knowledge of aurora but I do not count them mis-spent hours. If you have never seen aurora, the fine drawings by Mr. John R. Bell of Newcastle which are on view in the library will give you an impression of its beauty.

There is precedent for the choice of aurora as the subject of a lecture to our Society. Dr. Chree selected it for his Presidential Address in 1923, prompted no doubt by the recent establishment of a magnetic observatory at Lerwick. In concluding his lecture, Dr. Chree surveyed directions which future auroral research might take. He included every line which in fact has been pursued, including the influence of aurora on radio communication, which few could have foreseen at this time.

2. History

Appearances of aurorae, their awe-inspiring beauty and weird movements, have excited the interest of man from early times. I propose first to trace briefly the slow accumulation of knowledge by observation through the centuries to its culmination in the great work of Carl Störmer and then to survey current developments.

It is appropriate that what are apparently the first recorded references to aurora should appear in a work entitled *Meteorologica*, but on reading the text of the English translation (Webster 1923) of Aristotle's classic, one cannot be quite certain that the brief descriptions it contains are in fact of aurora, though they correspond in some respects to certain auroral forms. There can be little doubt however that Seneca in his *Quaestiones Naturales* is referring to aurora when he describes a ' luminous crown ' (Fig. 1, Plate XIV) and tells how a red glow on the horizon above Ostia caused the cohorts of Tiberius to rush northwards to assist in fighting what they assumed to be a fire. Much more recently fire engines have made the same unnecessary journey. Some centuries later, Gregory of Tours describes what is unmistakably a corona : ' There was in the middle of the heavens a cloud of light towards which all the rays converged in the form of a tent.'

In the Middle Ages when the practice of astrology had become rampant, it is not surprising to find that every great aurora that appeared over Central Europe excited superstitious fears (Figs. 2 and 3, Plates XV, XVI). Aurora held no terrors for the people of Scandinavian countries; they preferred to seek the cause of what was to them a familiar occurrence. In the Norwegian chronicle (*c.* 1250) *The King's mirror*, it is written, ' The men, who have thought about and discussed these lights have guessed at three sources, one of which, it seems, ought to be the true one.' It is thought to be either the reflection of the sun when it is below the horizon or the reflection of the fires that encircle the oceans, but ' There are still others who believe (and it seems to me not unlikely) that the frost

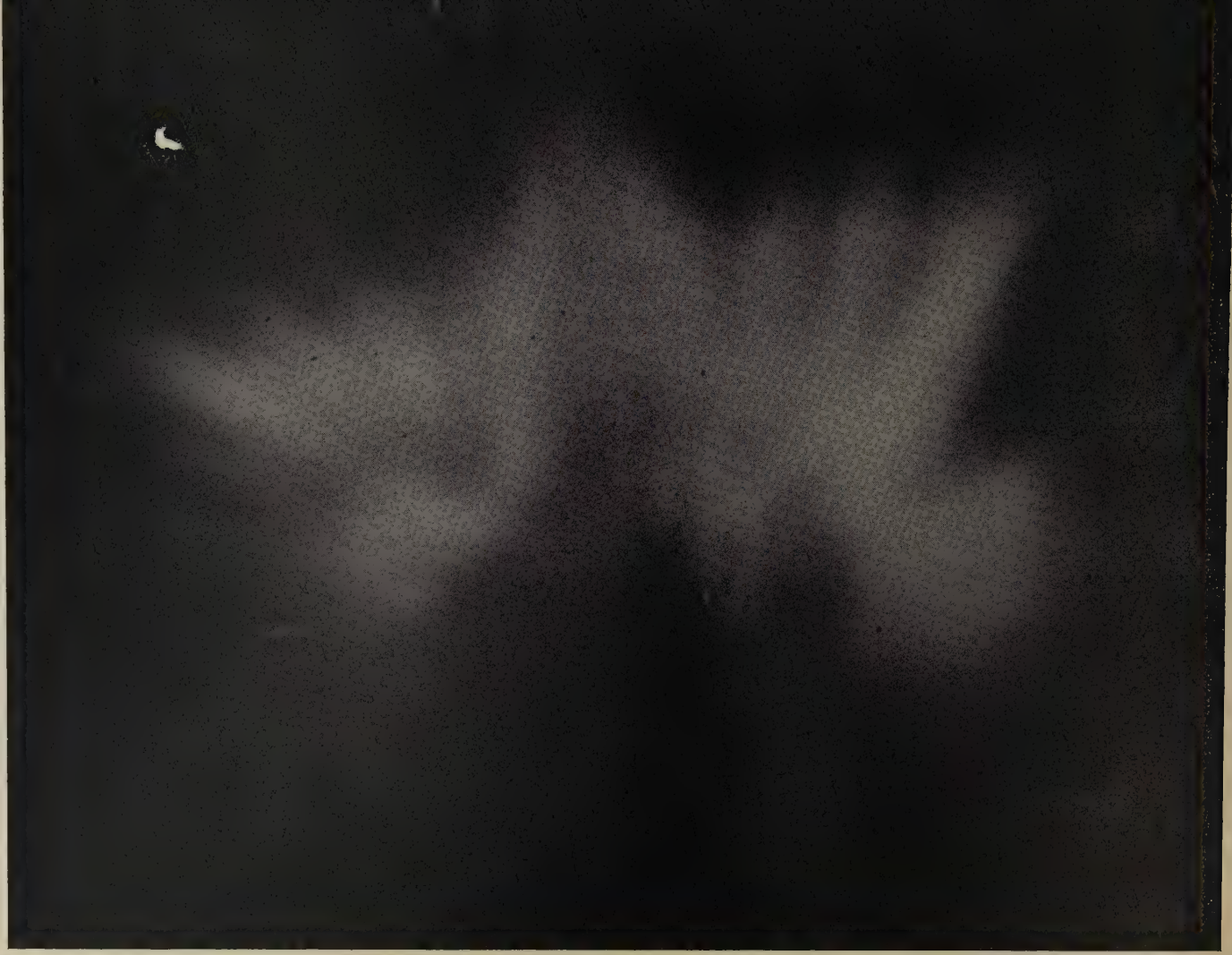


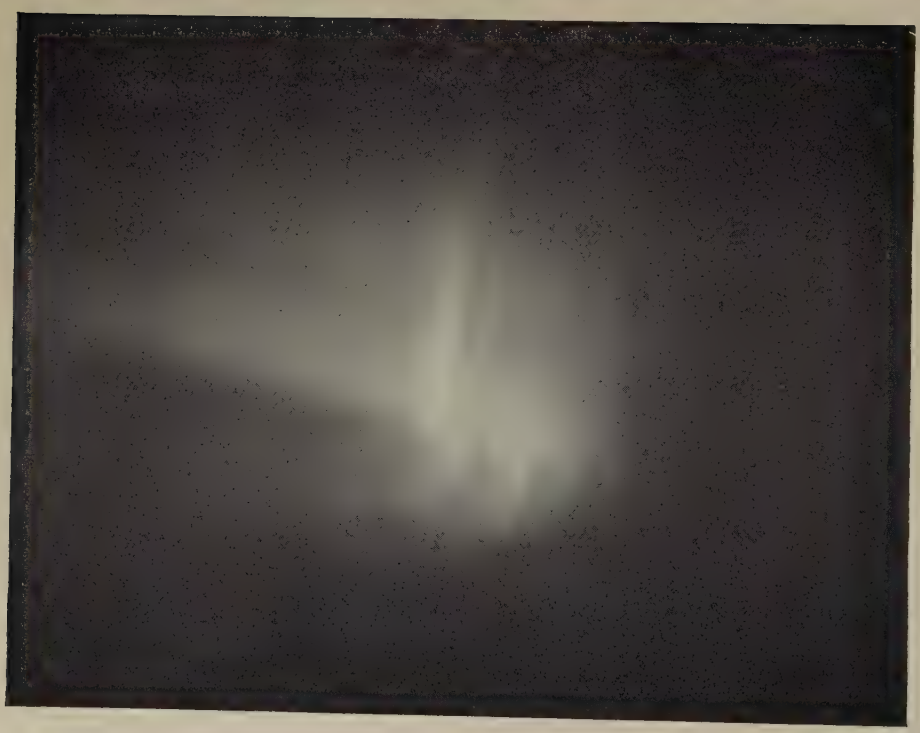
Figure 1. Auroral corona. The ' luminous crown ' of Seneca.

Ein vnerhörtes Wunderzeichen/welches iſt geſehen worden
auff Kuttenberg/in der Kron Böhem/auch ſonſt in andern Stätten vnd Flecken herumb/
den 12. Januarij/vier ſtund in die Nacht/vnd gewehret biß nach 8. Inn der Wolcken
des Himels ſtehen/ alß in diſem Jar. 1570.

Figure 2. A 'shocking portent' observed at Kuttenberg in the Kingdom of Bohemia for four hours during the night of 12 January 1570. An original print in the Crawford Library of the Royal Observatory, Edinburgh (reproduced by permission of the Astronomer Royal for Scotland).

Figure 3. Rayed arcs. The candles in Plate XV presumably represent rays of a rayed arc.

(a) Homogeneous arc.



(b) Quiet arc recurving.

Figure 5. A quiet arc recurving and changing to a curtain.

and the glaciers have become so powerful there, that they are able to radiate forth these flames.' Another translator renders this sentence, ' For my part, I think it is produced by the ice which radiates at night the light which it has absorbed by day.' This view was apparently held much later by both Descartes and Sir John Franklin.

It was a colleague of Descartes, Gassendi, who in 1621 first used the name aurora borealis in his *Physicks*. The aurora of the southern hemisphere, then called aurora australis, was observed and first reported by mariners shortly afterwards (1640).* The first detailed and accurate account of the development of a great display appears in a paper by Edmond Halley in *Philosophical Transactions of the Royal Society* (1716). He proposed that aurora was a luminous magnetic vapour constrained to move along the lines of force of the earth's field. This is surely the first realization of the association between aurora and the earth's field, though it is usually credited to Celsius and Hiorter, (1747) who first actually observed the perturbation of the magnetic needle during aurora. Later Wilcke (1777) in Sweden observed that auroral rays are parallel to the magnetic inclination. As early as 1733, Mairan was aware of the association with sunspots.

In his *History and present state of electricity* (1775) Joseph Priestley writes, ' That the aurora borealis is an electrical phenomenon was, I believe, never disputed from the time that lightning was proved to be one.' He quotes the opinion of one Beccaria of Turin (' whose observation and experience with respect to lightning give a weight to his opinion superior to that of any other man whatever ') ' who thinks that aurora borealis may be the electric matter performing its circulation in such a state of the atmosphere as renders it visible.'

That eccentric genius Henry Cavendish in 1784 calculated the height of aurora by observations of altitude at three stations in England and found it to lie between 52 and 71 mi. In 1817, Biot came to Scotland with his polarimeter, sought out aurora in Shetland and found its light to be unpolarized and therefore not reflected. It was Ångström who in 1866 showed that the spectrum consists of lines and bands and observed the famous green line which was to remain unidentified until 1925.

The many arctic expeditions of the 19th century, culminating in the International Polar Year of 1882-83, provided detailed information of auroral forms. That great observer, Bravais, records in his diary (*Recherche* expedition to N. Norway, 1837-40) what has at one time or another troubled every auroral observer, the difficulty sometimes of distinguishing between cirrus cloud and aurora.

' 11·45. To the NW a yellowish light in the form of flattened arc.
11·57. The flattened arc is decidedly cirrostratus.
12·15. The cirrostratus simulates the arc of an aurora.'

At this time, Hermann Fritz of Vienna, with unremitting energy, was searching world archives for accounts of aurora. He published (1873) a catalogue of occurrences for the N. hemisphere from 503 B.C. to 1870 A.D. and for the S. hemisphere from 1640 to 1872. The famous chart of isochasms appeared later (1874) and showed clearly the auroral zones.

3. The work of Birkeland and Störmer

Hittorf and Goldstein had now begun experimenting with cathode rays, so, when having observed a display apparently following the motion of the sun, Donati suggested that aurora might be caused by electric currents emitted by the sun, Goldstein (1881) proposed that the currents might consist of cathode rays. Paulsen (1894) put forward

* Professor Chapman is now of the opinion that the first use of the term aurora australis to describe an aurora of the southern hemisphere was by Captain Cook, when referring to an aurora he observed on 17 Feb. 1773 in position 57°8'S, 80°59'E.

the idea that the cathode rays originated in the upper atmosphere through the action of sunlight. A similar idea was put forward by Villard (1906), who attempted to explain thin auroral streamers by assuming that the cathode rays came from cirrus clouds. Birkeland showed that a source, limited to the day side of the earth and situated so close as proposed by Paulsen, could not account for the known distribution of aurora; the corpuscular rays must originate in the sun and be drawn in towards the polar regions of the earth. He therefore attempted (1901) to reproduce the production of aurora in the laboratory by placing a sphere with a core of iron and equatorial magnetising coil in a large discharge vessel. This magnetised ' terrella,' simulating the earth, was exposed to a stream of cathode rays, whose precipitations on the sphere were made visible by the fluorescence of a coating material. By suitably adjusting both the magnetising current and the speed of the cathode ray stream, the precipitation could be made to assume the form of well-defined spirals, one round each magnetic pole. Though the validity of interpreting this remarkable result as a close experimental reproduction of a large scale geophysical event has been questioned, it provided Birkeland with the stimulus to undertake his well-known work of analysis of magnetic disturbances (1908, 1913). Using the recordings of magnetic stations which he established in the auroral zones he showed that the magnetic disturbance in polar storms was caused by electric currents circulating, sometimes in a narrow belt, at heights of a few hundred kilometres. He believed the currents to consist of streams of electrons from the sun. It is now believed, of course, that it is the increased ionization of the air, following the passage through it of the solar corpuscular stream, which is responsible. Balfour Stewart (1882) had shown as early as 1878 that the movement of a conducting upper atmosphere across the lines of force of the earth's field would generate electromotive force and current.

It should be mentioned here, for though it is out of chronological order it is important for what follows, that Chapman has shown that the current system includes intense ' electrojets ' in the auroral zones, and flows westwards on the morning side and eastwards on the evening side of the earth, the circuit being completed in the atmosphere across the polar cap. Besides the two strong disturbance maxima in the auroral zones, there is a weaker one at the equator, a decrease in the normal equatorial field, which may be explained as due to a ring current encircling the earth in the equatorial plane. During great storms, the auroral zones are displaced towards the equator.

It was Birkeland's experiment that in 1903 caused Carl Störmer, then the newly-appointed Professor of Pure Mathematics in Oslo, to desert the theory of numbers for aurora. So impressed was he by the experiment that he resolved (1904) to determine mathematically the trajectories of an electron moving in the field of a dipole. Poincaré (1896) had just found the trajectories (geodetic lines on a cone with pole at the vertex) in the case of an isolated pole by integrating the equations of motion. The integrations in the case of a dipole have to be done numerically and involve laborious computing. The trajectories have complicated forms and to assist visually in assessing their effects, Störmer has constructed wire models. Most of the trajectories are deflected back from the earth on approaching to a certain minimum distance. Their effect in the mass is to contribute to a westwards directed ring current in the equatorial plane, perhaps at moon distance. This decreases the field parallel to the dipole axis as required. Those particles which reach the earth, precipitate in two zones round the poles to cause aurora and the polar storms. He showed that an increase in the ring current causes the zones of precipitation to move to lower latitudes. If trajectories corresponding to different positions of the sun with respect to the earth and starting parallel to the same meridional plane are drawn, the precipitation on the earth is in the form of a spiral current. So there is quite remarkable agreement between Birkeland's experiments and Störmer's theory, but the objection to

their application to aurora is that a stream of particles all of the same sign, and of sufficient density to cause the observed geomagnetic disturbance and aurora, would be dispersed by mutual electrostatic repulsion long before it arrived at the earth. More recent theories, which will be discussed later, are based on the assumption that the solar stream is electrically neutral, consisting of ions and electrons.

As his theoretical work developed, Störmer found it necessary to obtain more information concerning the height and disposition in space of the various auroral forms than was then available. He happened to be an enthusiastic photographer and in 1909 began experimenting with objectives and plates. During the next year he made his first measurements by taking simultaneous photographs at the end of a measured base line in Northern Norway (1911). At the age of 81, he is still engaged in this work, having accumulated something of the order of 10,000 pairs of measurable photographs. Lower limits of auroral forms lie mainly between 80 and 140 km with a pronounced maximum at just over 100 km. The average height of the lower limit for quiet forms and those without ray structure shows no significant variation with latitude. For those with ray structure, the average height of the lower limit increases as we pass to lower latitudes and there is also a marked variation in the vertical extent of the luminosity; rays in the auroral regions seldom extend much above 200 km, but in lower latitudes, especially near sunset and sunrise, they may extend to a height of over 1000 km during a great aurora. They are situated in the sunlit atmosphere, are usually of a feeble white-violet colour and their lower limits lie along the shadow line between the dark and sunlit atmosphere. Certain cloud-like aurorae, red diffuse surfaces, may appear at very great heights in the *dark* atmosphere during great aurorae. Using a base line of over 100 km, we found these red patches during the great display of 25 January 1949 to be situated at a height of over 700 km over Germany (Paton 1949). Störmer found that there is a diurnal variation in the height of the lower border of certain auroral forms, the maximum occurring just before midnight and the minimum 2-3 hours after midnight; diferences lie between 10 and 30 km.

So, since Carl Störmer was attracted to the study of aurora after watching Kristian Birkeland perform his experiments in the laboratory in Oslo in 1903, he has during more than half a century made a monumental contribution to our knowledge of the phenomenon. It is fitting that the Clarendon Press (1955) should recently have published his *magnum opus*, embodying his life's work, in its series of *International Monographs*.

4. Ionospheric relations

The photographic study of aurora is hampered by clouds and by twilight and moonlight. It is impossible by day, and there can be little doubt that aurora is present by day, for its accompanying magnetic storm proceeds day and night. A new method of detecting daylight aurorae and those concealed by cloud is at present being developed.

That an increase in ionization below the E-layer is an accompaniment of aurora was first shown by Appleton and his colleagues (1937) at Tromsö during the Polar Year of 1932-33. This precludes investigation of ionospheric conditions during aurora by conventional probing methods using frequencies in the range 1-20 Mc/sec, for there is strong attenuation in the dense ionization of the lower ionosphere. It was soon discovered however that echoes could be obtained during aurora by using higher frequencies in the range 30 to 150 Mc/sec. Professor Lovell and his colleagues (1947) at Jodrell Bank obtained echoes that appeared to come from a luminescent cloud at the end of an auroral ray and subsequently similar observations have been made in N. America, Sweden and Norway. There is as yet no general agreement concerning the interpretation of these echoes. Harang and Landmark (1954), finding no correlation between the range of the echoes and the

observed position of auroral forms, do not believe that these are true echoes from aurora. They propose that they are back-scatter from the earth's surface by way of the reflecting surface provided by the abnormal E-layer associated with the aurora. The majority of the observations suggest that the echoes arise either (1) from total reflection at dense isotropic clouds of ionization or (2) by scattering from numerous columns of ionization, similar to meteor trails, that may constitute an auroral ray. (Booker, Gartlein and Nichols 1955; Booker 1956).

As Professor Lovell pointed out in his Symons Memorial Lecture last year, the phenomena are complex and little progress has yet been made in the correlation of radio echo and visual observations. The rate of occurrence of radio echoes, for example, is at a minimum just at the time of day when aurora is most frequently observed.

5. Solar relations

(a) The most direct evidence of the solar connection of aurora and the magnetic storm is the occurrence of both of these phenomena about 24 hours after an intense flare has occurred in the central portion of the sun's disk. At the time of the flare, an area of the sun sometimes 100×10^6 mi^2 in extent suddenly flares up to ten times its normal brilliance in hydrogen light. The flare is usually associated with a sunspot, reaches its maximum intensity in a few minutes and thereafter decays in about half an hour. Simultaneous with the flare occur a short-wave radio fade-out and magnetic crochet, both limited to the sunlit hemisphere and therefore undoubtedly caused by a burst of ultraviolet light from the flare, which produces intense ionization even below the E-layer.

The delayed effects, the magnetic storm and aurora, are most likely to occur if the flare is within the central half of the sun's disk and seem almost certain to be caused by the emission of a corpuscular stream from the sun. If they follow a straight path to the earth, the corpuscles have a speed of the order of 1500 km/sec. The accumulating evidence suggests that every great aurora has its origin in an intense flare. It is the great aurora that becomes visible in low latitudes, exceptionally even as far south as geomagnetic latitude 10°.

(b) The much more frequent moderate aurora follows, on a smaller scale, the same general pattern as the great aurora, showing much ray activity and often flaming in its later stages. The duration is in general less than the great aurora and it does not move so far from the normal auroral zone, the southernmost point from which it is seen usually lying between geomagnetic latitudes 55° and 60°*. Its general resemblance to the great aurora and the fact that it also tends to occur when there are large sunspots near the central meridian suggest that it, too, is caused by a flare of lesser intensity, associated with the passage of the spots.

(c) During the years around sunspot minimum, aurorae appear with unexpectedly high frequency as far south as geomagnetic latitude 58°*. We have found that these aurorae are of different character from the great and moderate aurorae associated with the presence of sunspots near the central meridian (Paton and McInnes 1953, 1954, 1955). They almost invariably take the form of quiet and very diffuse homogeneous arcs, seldom showing ray activity. They appear in 27-day recurrence sequences, often of long duration; an occasional recurrence in the sequence may be visible as far south as geomagnetic latitude 55°. The accompanying magnetic storm, unlike the storms associated with great and moderate aurorae, shows no sudden commencement. Bartels (1934) suggested that these recurrent storms are caused by a particle stream which continues to be emitted for a long period from a restricted area of the sun, which he called a ' magnetically active '

* These limits were determined from observations made over the period of the last sunspot maximum (1947) and minimum (1954).

or M-region. The stream, sweeping round as the sun rotates, would spray across the earth every 27 days (the synodic period of rotation of the sun), causing these recurrent aurorae and storms. The M-regions have never been astronomically identified. It has been suggested that the white auroral streamers which are visible only during total eclipses and have their greatest extension in equatorial regions at sunspot minimum, may be streams of corpuscles. Analysis of observations made between 1953 and 1955, i.e., just over the period of sunspot minimum, showed recurrent aurorae, with some interruptions, during as many as 22 solar rotations (Paton 1956).

Greaves and Newton (1928) concluded that ' There seem to be grounds for suspecting that storms with sudden commencement are more closely connected with sunspots than those without.' The auroral evidence therefore bears out this suspicion in relation to the associated aurorae and further, provides support for Bartels's suggestion (1940) that there are two kinds of corpuscular stream, the ' nascent ' stream originating in a flare and the ' mature ' stream associated with M-regions.

(d) It seems likely that the aurora that is visible on every clear night in the auroral zones is caused by a continuous emission from the sun of a corpuscular stream, perhaps of low speed and density so that its precipitation is limited to the zones.

The variation of auroral frequencies with the 11-year sunspot cycle is therefore well defined only south of geomagnetic latitude 55° (Fig. 4), where sequence aurorae are not often visible. The maximum tends to occur one to two years after the sunspot maximum, perhaps for the reason the sunspots are then closer to the solar equator and solar streams therefore more likely to strike the earth.

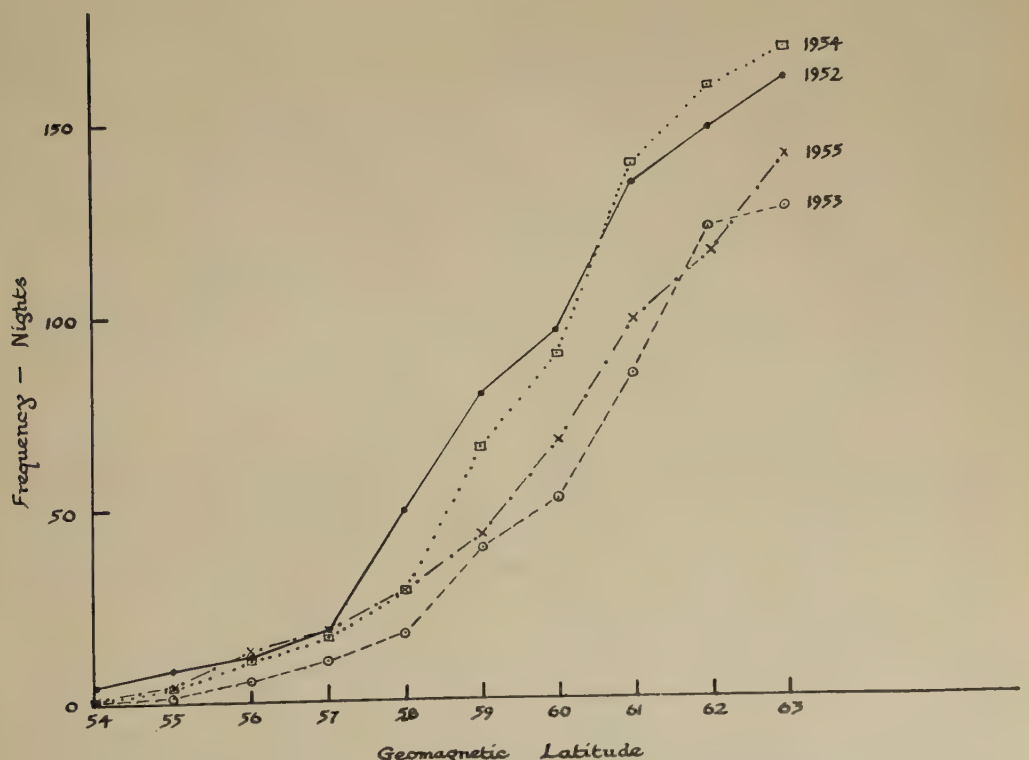


Figure 4. Auroral frequencies plotted against geomagnetic latitude.

6. The spectrum

Investigation of the auroral spectrum not only provides information concerning the composition and properties of the atmosphere at the levels where auroral light is emitted but also sheds light on the origin of aurora itself. Besides, analysis of the spectrum is particularly valuable to the spectroscopist in that it may yield new knowledge of atomic and molecular spectra and of collision processes, for the conditions of emission in the upper atmosphere cannot easily be reproduced in the laboratory. For example, forbidden emissions, that is, emissions associated with transitions that violate the selection rules, are prominent in the auroral spectrum. Before undergoing an allowed transition and emitting a permitted line, an atom remains in the excited state for about 10^{-8} sec. But before a forbidden transition, the atom must remain on the average in the excited state – a metastable state – for a period of the order of seconds, so low is the transition probability associated with this state. In laboratory conditions, most atoms in metastable states are deactivated by collision with other particles or with the walls of the discharge tube before they may radiate. In the upper atmosphere the long time interval between successive collisions may permit many atoms in metastable states to finish their lifetimes by spontaneous transition and the emission of forbidden lines. It is therefore not surprising that the identification of the prominent auroral green line 5577Å as a forbidden line of atomic oxygen was delayed until 1925 (McLennan and Shrum 1925).

The complexity of the spectrum with its overlapping band systems often obscuring important lines adds further to the difficulties. The established emissions from atmospheric constituents consist of permitted and forbidden lines of neutral and ionized atomic oxygen and nitrogen and band systems of neutral and ionized molecular oxygen and nitrogen.

The possible processes responsible for the various emissions are numerous and have recently been examined by Bates (1955). The variation of the light intensity with height in most auroral forms is consistent with the assumption that the light is produced by excitation and ionization of the atmospheric gases, during the passage down through them of a stream of corpuscles. The extent of the ionization and excitation depends both on the speed of the particles and on the air density. So, as the speed of a corpuscle decreases on penetration, the excitation and ionization which it causes will increase or decrease according as the speed of the corpuscle is above or below the critical speed for maximum ionization or excitation. Secondary electrons ejected from atoms and molecules are likely to play an important part in excitation and ionization; primary electrons in the incident solar stream would require to possess speeds much greater than that of the accompanying ions if they are to cause much luminescence. In the case of sunlit aurorae, direct solar electromagnetic radiation must play some part in the process of emission.

The most striking advance in recent years has been the identification of emissions of lines by elements present in the incident solar stream that causes aurora. This has been made possible by the use of modern spectrographs of great light-gathering power and dispersion which provide measurable spectra of limited portions of the sky with exposures as short as 20 min. In this way, Meinel (1951) has been able to examine spectral characteristics of short-lived auroral features. He was the first to measure the Doppler shift of the Balmer lines of hydrogen (previously identified in the auroral spectrum by Vegard (1939) and Gartlein (1950)) and so to provide conclusive evidence that protons in the incident solar stream play an important part in the production of aurora. His measurement of the displacement of the H_α line in the spectrum of a quiet arc in the magnetic zenith during the display of 18-20 August 1950 indicated that protons travel into the auroral regions with speeds of 3,300 km/sec. The light emission takes place when the protons have been slowed down sufficiently by collisions to capture electrons;

the speed with which the protons enter the upper atmosphere must therefore be considerably greater than 3,300 km/sec. If it be assumed that the effective range of the protons ends at about 85 km at the base of the arc, then the vertical thickness of air they have penetrated is about 5 cm at STP. Their speed on entering the atmosphere must therefore be at least of the order of $1{\cdot}2 \times 10^9$ cm/sec and their energy at least about 750 k.e.v. This conflicts with the speed of 10^8 cm/sec calculated from the time lag after the flare.

Bates (1955) has shown that the H_α emission would in general be expected to occur only after the proton energy has been reduced to about 100 k.e.v. Thereafter the remaining range is no more than 0·1 cm of air at STP. so the H_α emission would be expected to be confined to a height range of about 3 km for an aurora whose base is at 105 km. The spectrographic results reveal a rather wider range. The discrepancy may be partly due to the fact that the velocity distribution in the incident stream of particles is wide and partly, as Bates points out, because the particles may not travel directly down the lines of magnetic force, as he had assumed; they may move down in spiral orbits round the lines of force. Indeed this mode of travel is suggested by Meinel's observation that the H_α profile in the spectrum of an arc observed on the magnetic horizon is broadened but *symmetrical*.

At all events, immediately a quiet arc becomes active and disintegrates into ray bundles, the H_α emission ceases, so that apparently protons play no part in the formation of rays. Meinel has in fact suggested that protons may be the primary excitation mechanism and that rays and flaming are delayed effects of the ionization and excitation produced by the initial proton stream. The cause of the transformation of the quiet arc to rays and curtains (Fig. 5, Plate XVII), so characteristic of auroral displays, remains a mystery.

Vegard (1948) has for long claimed that the *D* lines of sodium appear in the auroral spectrum, but until recently when Petrie and Small (1952) provided confirmatory evidence, it was thought to be merely the night-airglow background. Sodium is not plentiful in the solar chromosphere so is unlikely to be an important constituent in the solar corpuscular stream. Though Bates (1955) has shown that the photon emission per incident sodium ion is likely to be higher than the photon emission per incident proton, he stresses the rarity of sodium relative to hydrogen in the solar stream. On the other hand, lines of neutral and ionized helium, which Bernard (1948) claims to have identified, are thought to be doubtful.

Theoretical considerations indicate that temperatures of the upper atmosphere at auroral levels may be determined from spectral characteristics. The rotational and vibrational distributions have been used with results that were at first conflicting, but much progress has been made in recent years particularly by Canadian workers. Invaluable information concerning temperature might be obtained from the determination of the Doppler broadening of forbidden atomic lines. For this work, interferometers of great resolving power will be necessary. Jarrett of St. Andrews has made preliminary experiments on the green line of atomic oxygen in which he replaces the silvered films in the Fabry-Perot interferometer with multi-layer coatings (5 quarter-wave dielectric layers).

7. Theories of aurora

For reasons already stated, the more recent theories of aurora are based on a neutral solar stream, consisting of ions and electrons. Chapman and Ferraro (1954) have examined mathematically the behaviour of a neutral stream as it approaches and sweeps past the earth. As the highly conducting stream approaches the earth, electric currents are induced in its surface by the geomagnetic field, the interior of the stream being thus shielded from

the influence of the earth's field. While the stream is still some distance from the earth, it is shown that the field outside it, due to this induced current system, is in the direction of the earth dipole. This provides an explanation of the first phase of a magnetic storm as recorded in the regions of the earth between the auroral zones, namely, a sudden increase in the horizontal force.

Further, by calculating the induced currents, the force retarding the advance of the stream may be determined and it is shown that the field carves out a hollow in the stream. As the hollow grows, deflection of the surface charges under the influence of the earth's field causes the wall opposite the morning side of the earth to become positively charged and opposite the evening side to be negatively charged. Chapman and Ferraro then suggest – the theory has not been continued mathematically beyond this point – that the main phase of the storm, during which the horizontal intensity falls and remains below the normal value, may be explained by assuming that when the hollow has completely enveloped the earth, positive surface charges on the morning side leap across the gap to start a westwards current flowing round the earth. It is the magnetic field of this ring current which is thought to be responsible for the reversal of the field change set up in the initial phase.

Aurora is thought to be caused by the repulsion of charges from the hollow. The charges are then directed under the influence of the combined earth and ring current field towards the poles, where they produce aurora. It will be noticed that whereas in the Störmer theory electromagnetic deflection of the particles begins at a great distance from the earth, it is here confined to the region within the hollow.

While also basing his theory on a neutral solar stream, Alfvén (1950) treats the problem in a quite different manner. He assumes the existence of a general magnetic field of the sun. If a particle stream is emitted from a limited region of the sun, the electrons and ions both spiral outwards round the lines of force but drift in opposite directions because the field decreases outwards. The lateral surfaces of the stream thus become oppositely charged and an electric field develops across the stream (eastwards). This field acting with the solar magnetic field (northwards) drives the particles radially outwards. He calculates separately the paths of the two sets of charges in the vicinity of the earth and shows that they eventually flow together along the auroral zones. Aurora is regarded as a gaseous discharge occurring in the electric field of space charges of opposite sign, associated with the streams. The discharge may carry current between space charges at a great distance from the earth perhaps to the ionosphere. Certainly many auroral features, for example, flaming, show a similarity to laboratory-produced electrical discharges. Alfvén cites the case of an auroral ray which may remain apparently fixed in position with respect to an observer for a considerable time. It would be regarded on the other theories as produced by a thin stream of corpuscles, and it is difficult to see why such a stream entering from space should partake exactly of the earth's rotation. On the discharge theory, once the current causing the ray has begun to flow, it will continue to follow the same ionized path. But the theory has been severely criticised on the grounds that electrostatic fields resulting from the space charges would so alter the paths of the charges as to vitiate the entire theory.

The whole problem is of the utmost difficulty and little progress has been made towards a satisfactory theory of aurora.

8. Auroral morphology

Professor Chapman has pointed out that theoretical investigations would benefit from a greater knowledge of what he calls auroral morphology – the study of the geographical distribution of aurora and of the details of changes in form, colour, spectrum and

geographical position that occur during the course of a great display or during the year or the solar cycle.

Much of the region over which aurora occurs, especially in the S. hemisphere is uninhabited and inhospitable and, in consequence, detailed information of the area covered by each display and the changes in character during a display is lacking. The necessary information can be obtained only from the observations of a close network of trained observers situated within the regions where aurora is visible. It is plainly quite impossible to establish this ideal network in adequate density over the whole of the auroral zones in each hemisphere.

We have attempted to organize a network within the region bounded by geomagnetic longitude lines 70° and 90°, extending from N. Greenland across the auroral zone to the south of England. The nucleus of land observers consists of members of the Aurora and Zodiacal Light Section of the British Astronomical Association and the numbers were considerably increased in response to appeals for observers which appeared in scientific journals and magazines. The Directors of the Meteorological Office and of the Meteorological Services of Ireland and Iceland granted permission for observers at certain stations to make brief reports at each observing hour and it was also arranged that similar observations be made at sea in selected ships. Members of night-flying squadrons of the R.A.F. and officers of civil air lines make observations during flights in the prescribed area. In particular, very valuable and regular reports are provided by crews of B.O.A.C. transatlantic aircraft. Between 1952 and 1954, members of the British North Greenland Expedition made observations within 10° of the geomagnetic pole.

Special types of form for recording observations and instructions are supplied for land, sea and air observations. Observers are asked to report whether aurora is present *or* absent *or* if cloud, moonlight or twilight makes observation doubtful or impossible. If aurora is present, the forms and their position and luminosities are reported. Observers who are free to observe continuously over a period during a display are asked to record their observations according to a standard pattern. They are being supplied with alidades for accurate observation of elevation and goggles fitted with interference filters, transparent to a narrow band of wavelengths centred on the green line, so that they may identify weak aurorae and those concealed by twilight or moonlight. Completed forms are returned at the end of each month.

The observations are plotted for each night on a chart, an example of which is shown in the figure (Fig. 6). The entry is in red ink if aurora is present, green when it is overhead, symbols being used to represent the various auroral forms and figures to indicate luminance and elevation. A line in blue ink indicates the absence of aurora and a pencilled line signifies that cloud or some other obscuring factor makes quite impossible the observation of any aurora that may be present. The completed diagram provides a synoptic picture of the aurora and shows its movement and development throughout the night as observed within the region from Iceland to the south of England. The time scale is chosen to coincide with that of the magnetograms recorded at Lerwick and Eskdalemuir, which are entered on the diagrams; solar and ionospheric information is also entered. Our aim is to prepare a complete set of these charts for a period of one solar cycle.

9. The analysis of the observations

The isochasmic chart of Fritz shows the frequency of visibility of aurora from different places on the earth. An aurora seen on the horizon may be overhead at a distance of as great as 1,000 mi, i.e., about 10°, from the observer, so that the frequency plotted for a particular place includes all displays occurring within a belt of latitudinal width 20°.

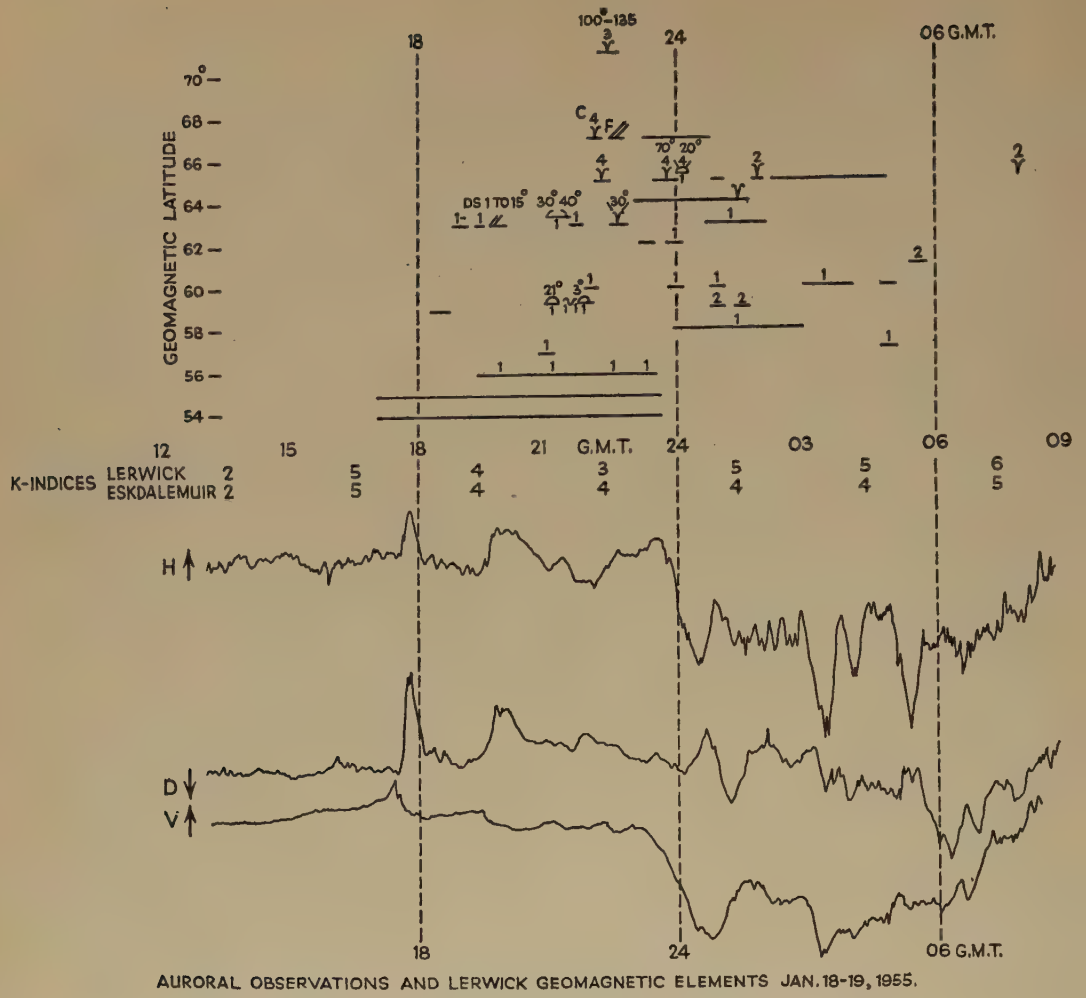


Figure 6. The method of plotting observations made by observers of the Aurora Survey.

Chapman (1953) has pointed out that a much more precise representation of the geographical distribution of auroral occurrences would be provided by a map containing lines, isoauroral lines, at all points along which the frequency of occurrence of *overhead* aurorae is the same. Such diagrams may be easily prepared from information contained in the plotted charts. The diurnal and annual variation in different latitudes may be readily determined as well as any movement of the auroral zones during the sunspot cycle.

The graphical method of representation of the observations is particularly suited to investigation of the solar, magnetic and ionospheric relations of aurora. It is not yet known if there exists a relation between the auroral forms and the accompanying pattern of the magnetic disturbance; if the aurora associated with a storm with sudden commencement begins equally abruptly and if these two events are simultaneous; and if any significant change in the character of aurora occurs at the time when the storm passes from the first into the main phase. Dr. Stagg and I (1939) found by examining the photographs of quiet arcs and the associated magnetograms of the British Polar Year Expedition led by Dr. Stagg to Fort Rae in 1932-33, that there exists a close spatial association between the current system responsible for the magnetic disturbance and the

auroral arc. In one case, as the arc passed overhead there was a simultaneous change in the sign of ΔZ. It has already become clear that while the most active aurorae are invariably accompanied by great magnetic activity, the relationship is by no means close when the magnetic disturbance and aurorae are only moderate. It is important to examine the not infrequent cases when aurora occurs in magnetically quiet conditions and is absent in moderately disturbed conditions.

10. The international geophysical year

During the Geophysical Year of 1957-8, aurora will be observed more intensively and systematically than ever before. In particular, the many antarctic expeditions will furnish information concerning aurora australis, which will permit for the first time adequate examination of its correlation with aurora borealis. At this time solar activity will be near maximum and great aurorae may be expected. A continuous watch on the surface of the sun will be kept at solar observatories, and observers all over the globe will be warned by a Prediction Centre when solar activity indicates that a widespread aurora is more likely than usual.

It is hoped to extend the network of visual observers southwards into France and Spain in preparation for great aurorae. A modern auroral spectrograph, kindly lent by the Cambridge Air Force Research Center, U.S.A., has been installed at the University Observatory, St. Andrews, as well as an interferometer. Parallactic photography will be carried out at three stations in Scotland and will provide height measurements of the features being examined by the spectroscopists. Radio-echo investigations will be continued at Jodrell Bank and equipment in preparation for similar work is presently being built at St. Andrews. An all-sky auroral camera, which will automatically photograph the whole sky at minute intervals, and a colorimeter will also be operated at St. Andrews. Both the British Antarctic Expeditions – the Royal Society and the Transantarctic – will make visual observations of aurora australis and an all-sky camera has already been set into operation by Dr. Stanley Evans at the base of the former in Coats Land (Halley Base).

We may confidently hope that the mass of information that will accrue from this great international plan will in some measure assist in unravelling the tangle of problems presented by aurora.

Acknowledgments

It is a pleasure to acknowledge the assistance of the many observers who continue to make nightly observations for the Aurora Survey on land, at sea and in the air, and of Mr. J. B. McKellican and the Rev. Dr. Cameron Dinwoodie who maintained photographic stations at Blairgowrie and Rosneath. Mr. G. V. Black has given invaluable advice and assistance in photographic matters since auroral work began in Scotland in 1938.

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The use of a stream function in a two-parameter model of the atmosphere

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SUMMARY

In an attempt to obtain a better approximation to the horizontal flow of a non-divergent fluid than the geostrophic approximation, a Monge-Ampère type of partial differential equation, similar to that of Charney (1955), is derived to enable a stream function to be found from the contour height field. A method is given for the numerical solution of this equation.

The stream function has been used in place of the contour height parameter to represent the flow at the mean level in computing three 24 hr forecasts based on the Sawyer-Bushby two-parameter model of the atmosphere, and the results are compared with similar computations without the stream function.

In one situation, the use of the stream function produced a marked improvement in the computation.

1. INTRODUCTION

Most of the recent work on numerical weather prediction has been characterized by the use of the geostrophic approximation to the wind field. An examination of about 20 computed forecasts based on the Sawyer-Bushby (1953) two-parameter model of the atmosphere suggested that in one or two situations the use of the geostrophic approximation might be responsible for a large part of the difference between computed and actual charts.

This paper describes an attempt to improve upon the geostrophic approximation by representing the wind field in an isobaric surface by a stream function which differs from the geopotential. The stream function is derived from an equation, similar to the balance equation derived by Charney (1955), which relates the horizontal velocity field at a given pressure level to the contours of the same isobaric surface. Three 24-hr forecasts have been computed by applying this technique to the flow at the representative middle layer in a two-parameter model, and the results have been compared with previous computations in which no attempt had been made to improve upon the geostrophic approximation.

2. THE DERIVATION OF THE STREAM FUNCTION

Since the significant motions of the free atmosphere are characterized by small horizontal divergences we can adequately express the velocity components u and v in terms of a stream function ϕ by

$$u = -\frac{g}{l}\frac{\partial \phi}{\partial s}, \qquad v = \frac{g}{l}\frac{\partial \phi}{\partial r} \quad . \quad . \quad . \quad . \quad (1)$$

where r and s are orthogonal curvilinear distance co-ordinates on the sphere. If the rather slow variation of the Coriolis parameter l with latitude is ignored, this velocity field is non-divergent.

Ignoring the vertical motion terms and the variation of l, substituting from Eq. (1) and taking the horizontal divergence of the equations of motion, we obtain (when pressure is used as the vertical coordinate),

$$\frac{2g}{l^2}\left\{\left(\frac{\partial^2 \phi}{\partial r\,\partial s}\right)^2 - \frac{\partial^2 \phi}{\partial r^2}\frac{\partial^2 \phi}{\partial s^2}\right\} - \nabla_e^2 \phi = -\nabla_e^2 h \quad . \quad . \quad . \quad (2)$$

where h is the height of the contour surface and ∇_e^2 is the two-dimensional Laplacian operator on the sphere. This equation is of the Monge-Ampère type and can be solved for ϕ as a boundary-value problem provided that

$$\frac{g}{l}\nabla^2 h + \frac{l}{2} > 0$$

Charney (1955) does not need to ignore the variation of the Coriolis parameter with latitude as he uses a stream function such that

$$u = -\frac{\partial \phi}{\partial s}, \qquad v = \frac{\partial \phi}{\partial r} \qquad . \qquad . \qquad . \qquad . \qquad (3)$$

He then derives an equation similar to Eq. (2) but including a term grad ϕ . grad l. Although Charney's equation can be derived with one less approximation than Eq. (2), it is more difficult to obtain suitable boundary values for ϕ when Eq. (3) is used to define the stream function than if Eq. (1) is used for that purpose.

Representing the earth's surface by a stereographic projection of the northern hemisphere on to a plane perpendicular to the earth's axis passing through the north pole, Eq. (2) becomes approximately

$$\frac{2\beta^2 g}{l^2}\left\{\left(\frac{\partial^2\phi}{\partial x\,\partial y}\right)^2 - \frac{\partial^2\phi}{\partial x^2}\frac{\partial^2\phi}{\partial y^2}\right\} - \nabla^2\phi = -\nabla^2 h \qquad . \qquad . \qquad (4)$$

where β is the magnification factor $\sec^2\left(\frac{\pi}{4} - \frac{\theta}{2}\right)$ and x and y are rectangular Cartesian coordinates on the plane. The ratio of the small terms neglected in deriving Eq. (4) from Eq. (2) to the smallest terms which appear in Eq. (4) is of the same order as the ratio of the wavelength of the characteristic patterns of the stream function to the circumference of the earth.

At first sight it appears that, since the geostrophic approximation is a good approximation to the wind field, we could obtain a solution to Eq. (4) by successive approximation, by repeatedly solving the Poisson equation

$$\nabla^2\phi_{n+1} = \nabla^2 h + \frac{2\beta^2 g}{l^2}\left\{\left(\frac{\partial^2\phi_n}{\partial x\,\partial y}\right)^2 - \frac{\partial^2\phi_n}{\partial x^2}\frac{\partial^2\phi_n}{\partial y^2}\right\} \qquad . \qquad . \qquad (5)$$

for ϕ_{n+1}, where the suffixes refer to the number of the approximation, using $\phi_0 = h$. However, in anticyclonic regions the approximation $\phi = h$ can imply winds of as little as half the actual winds, and in these regions the successive-approximation method described above often diverges.

Eq. (4) can be written as the following finite-difference approximation

$$\sum_1^4 \phi_i - 4\phi_0 - \sum_1^4 h_i + 4h_0 - \frac{2\beta_0{}^2 g}{a^2 l_0{}^2}\left\{\tfrac{1}{16}(\phi_A+\phi_C-\phi_B-\phi_D)^2 - (\phi_1+\phi_3-2\phi_0)(\phi_2+\phi_4-2\phi_0)\right\} = 0 \qquad (6)$$

where a = grid length and the suffixes refer to points relative to the origin 0 as shown in Fig. 1. Eq. (6) is a quadratic equation in ϕ_0 and can be re-written as

$$\phi_0 = \tfrac{1}{8}\Big\{4k + 2\sum_1^4 \phi_i \pm$$
$$\pm\left[16k^2 + 4\left(\sum_1^4 \phi_i\right)^2 + 16k a^2\nabla^2 h + (\phi_A+\phi_C-\phi_B-\phi_D)^2 - 16(\phi_1+\phi_3)(\phi_2+\phi_4)\right]^{\frac{1}{2}}\Big\} \qquad . \qquad . \qquad (7)$$

where $k = \dfrac{a^2 l_0{}^2}{2\beta_0{}^2 g}$.

It is clear that the solution we require is the one with the negative sign outside the square root; consider, e.g., the case of parallel, equidistant straight contour lines.

Figure 1. Suffix notation relative to origin 0.

Given ϕ on the boundary we can obtain a solution to Eq. (6) by making an initial guess at the answer (say $\phi = h$) and then using Eq. (7) in a systematic point-to-point manner over the grid, continuing the process until there is no longer any significant change in ϕ. Since the stream function is defined by Eq. (1) it is possible to use $\phi = h$ on the boundary as the boundary condition. This iterative process appears to be slowly convergent in all circumstances. 15 to 20 iterations were usually needed to obtain the necessary accuracy in the answer when an 18×14 grid was used.

The above procedure is not possible if the quantity under the radical (say A) in Eq. (7) is negative. In practice this difficulty was overcome by replacing a negative A by zero. If we consider a stream function ϕ consisting of a series of concentric circles then excluding negative values of A implies $\frac{g}{l}\nabla^2 h + \frac{l}{2} \geqslant 0$, the condition necessary for Eq. (2) to be solved as a boundary-value problem. With circular isobars this is equivalent to the condition that the absolute vorticity must not be negative.

In order to test this method of solving the Monge-Ampère equation, it was used to compute the stream function ϕ at 600 mb for three different synoptic situations. The computed values of the stream function were next substituted back into Eq. (4) and this was then solved as a Poisson equation for h. The field of h computed in this way showed very little difference from the actual field. The trough-ridge patterns were identical, but the absolute values of the computed height field were consistently less than the actual values. This difference, almost certainly due to the replacement of a negative A by zero, varied linearly from 100-150 ft in the centre of the area to zero on the boundary. The vorticity field associated with this difference is small compared with the actual vorticity field and therefore the results of time integrations based on these two h fields will be similar in pattern but different in absolute values, in the same manner as the initial fields. It is therefore probable that any change (apart from a general lowering of the absolute height values) in the result of a time integration, due to the use of a stream function instead of the contour field to represent the mean flow, is due to a different delineation of the wind and vorticity field and not due to the imposition that $A \geqslant 0$.

3. Application of the stream function to the two-parameter model

We can represent the horizontal velocity by a stream function only if the flow is approximately non-divergent, and therefore it would appear difficult to make use of a

stream function in any model other than the simple barotropic one. However, the two parameters h_m, h' of the Sawyer-Bushby model refer to the height of the mean level of the layer considered and the thickness of the layer. h_m is used solely to represent the flow at the mean level (which even in the two-parameter model is thought to be approximately non-divergent, consistent with the assumption of a parabolic vertical motion profile) and we therefore decided to replace h_m by ϕ_m in the prognostic equations. h' however, besides representing the wind shear, does in fact represent the mean temperature of the layer, and it was therefore decided to leave the parameter h' unaltered.

Three time-integrations were performed in which ϕ_m was first computed from the initial h_m. ϕ_m was subsequently used instead of h_m during the time integration, and finally h_m at time $t = 24$ was computed from ϕ_m at that time by solving Eq. (6) as a Poisson equation for h_m, given ϕ_m and boundary values for h_m. The boundary values of h_m were taken as $h_m = \phi_m$.

The particular form of the Sawyer-Bushby model used for this experiment was the one which took 600 mb as the mean level of the layer, included the effect of sea-surface heating, and used the actual changes that occurred on the boundary as boundary conditions. The results have been compared with time integrations not using the stream function but otherwise using the same version of the two-parameter model.

4. The results of the time integration

(a) *1500 GMT 27 January 1952*

The computation for this situation without use of the stream function has been discussed by Bushby and Hinds (1955).

As the introduction of the stream function has made little difference to the forecast charts they are not reproduced here. At the 500-mb level a closed circulation has been introduced over Scandinavia, but the position and central values are not very accurate. Over most of the area the introduction of the stream function has led to a reduction in the height of the 500-mb level by something of the order of 100-200 ft. This is not an improvement on the earlier computation and may have been brought about by a reduction in the total anticyclonic vorticity in the forecast area as a result of replacing a negative value of A by zero.

The use of the stream function has only had a slight effect on the thickness. Thus the difference between the two computed 1,000-mb charts is almost the same as the difference between the computed 500-mb charts.

(b) *1500 GMT 30 January 1952*

As the use of the stream function for this situation produced a significant change in the results, they are illustrated in Figs. 2 (a)-2 (h).

Although previous computations had been made for this situation they had been made with other variations of the Sawyer-Bushby model than that used here. (Bushby and Hinds 1954, 1955). Thus it is important to note that when this variation is used (without stream function) there are certain differences from the previous numerical forecasts. The trough from south of Iceland to the Faroes now turns down over the eastern North Sea (Fig. 2 (e)) rather than continuing eastwards over Norway and Sweden and there is now no cut-off 500-mb depression in the vicinity of the Faroes. The axis of the jet is now west-east over the British Isles. The thickness and 1,000-mb patterns (Fig. 2 (f)) are also slightly different from those produced in previous numerical forecasts.

The results of the calculations using the stream-function technique are given in Figs. 2 (g) and 2 (h). The 500-mb trough is now forecast to be over the western North Sea which is better than the forecast without the stream function. Also, the axis of the jet stream is further south, although still not far enough, and is orientated WNW-ESE. Fig. 2 (e) shows pronounced ridging over the southwestern seaboard of Europe whereas this is minimized to a large extent in Fig. 2 (g).

Again there is only very little difference between the computed thickness charts shown in Figs. 2 (f) and 2 (h), and the improvement in the 1,000-mb pattern in Fig. 2 (h) compared with the 1,000-mb chart shown in Fig. 2 (f) is similar to that already described relative to the 500-mb pattern.

(*c*) *1500* GMT *30 January 1953*

The synoptic situation in the vicinity of the British Isles at this time is of particular interest because within 24 hr large areas of east and south-east England and the Low Countries were being affected by a severe gale, and coastal flooding was the worst for many years.

Numerical integrations previously performed on these data (Bushby and Hinds 1954, 1955) had used 500 mb as the level representing the mean tropospheric flow and had used no change on the boundary as the boundary condition. In these earlier computations the main features producing the gale had been indicated quite well, but an anticyclone west of Ireland had been grossly exaggerated, leading to too strong wind gradients over the British Isles.

A time integration was therefore carried out using the 600-mb level to represent the mean flow and using the actual changes round the boundary as the boundary condition. These results show a marked improvement west of Ireland where the intensity of the surface anticyclone is much more realistic and almost correct, but an upper closed anticyclonic circulation is still wrongly forecast (although it is much weaker than in the previous computations) south-west of Ireland. The trough over the North Sea is still broadly correct at 500 mb, although the main trough line is now too far west and in this respect the previous computations were better, both at the 500-mb level and at the surface where the strong northerly winds are computed too far west over the British Isles. However, the position and intensity of the depression between Scotland and Norway is almost exactly forecast.

The result of using the stream-function technique in this particular situation resulted in the upper anticyclonic circulation almost completely disappearing. The region west of Ireland was correctly dealt with apart from the fact that the contour heights along the axis of the ridge were about 200 ft too low. This again was probably due to the fact that a negative A is replaced by zero, thus giving less anticyclonic geostrophic vorticity in the forecast area than there should be. The fact that this was not replaced at the end of the computation when the h_m field was computed from the ϕ_m field does imply that the mean value of h_m inside the forecast area is less than it should be. This again probably accounted for the depression east of Scotland being a little too deep.

In this particular situation a somewhat better answer (apart from the position of the 500-mb trough line near the British Isles) has been obtained by using 600 mb rather than 500 mb as the operative level and by using the actual changes along the boundary rather than assuming no change along the boundary. The introduction of the stream function technique has made a further slight improvement in the forecast in the anticyclonic region west of Ireland.

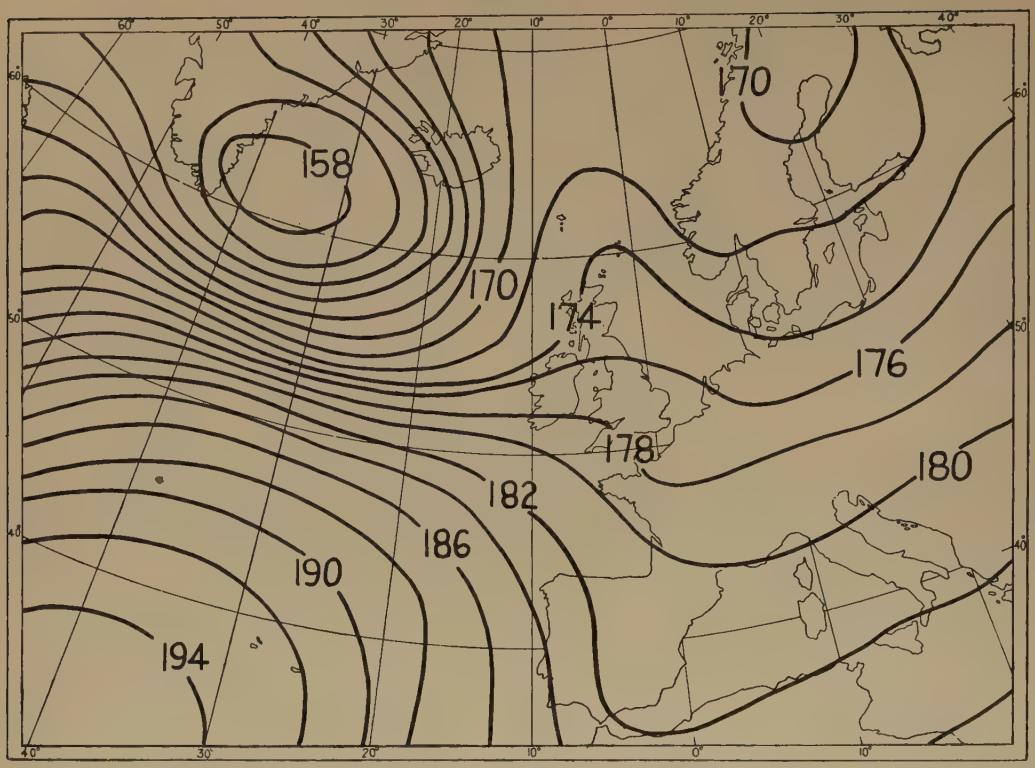

Fig. 2 (a). Actual 500-mb contour chart for 1500 GMT 30 Jan. 1952.

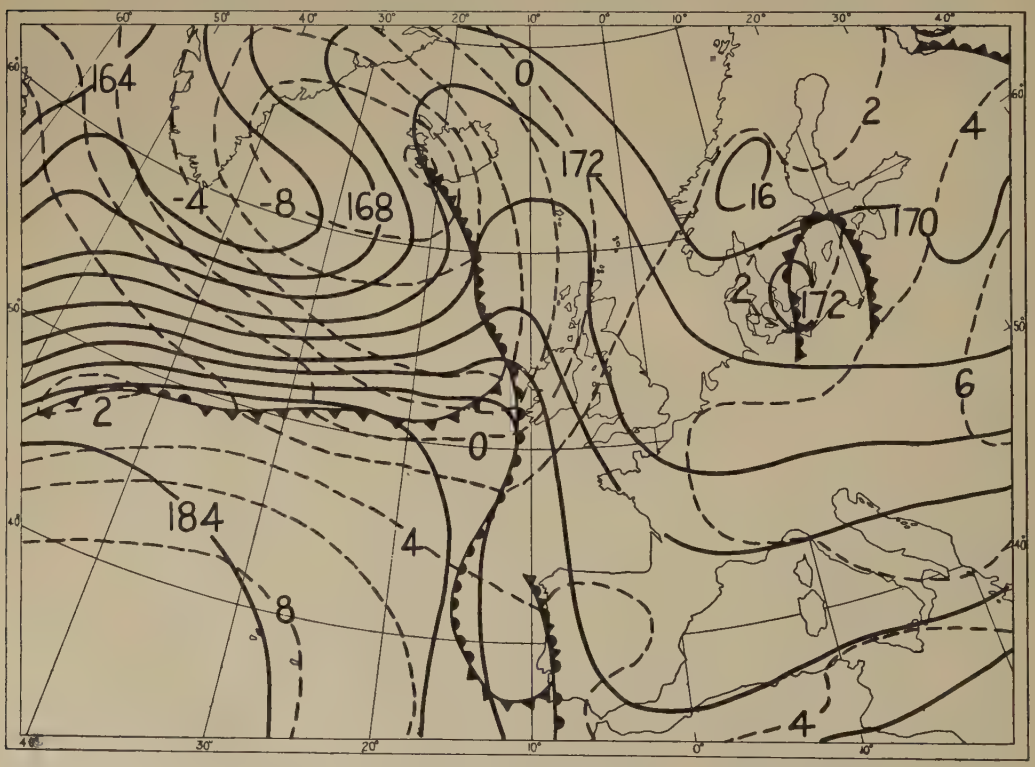

Fig. 2 (b). Actual 1,000-500 mb thickness (– – –) and 1,000-mb contour (——) charts for 1500 GMT 30 Jan. 1952.

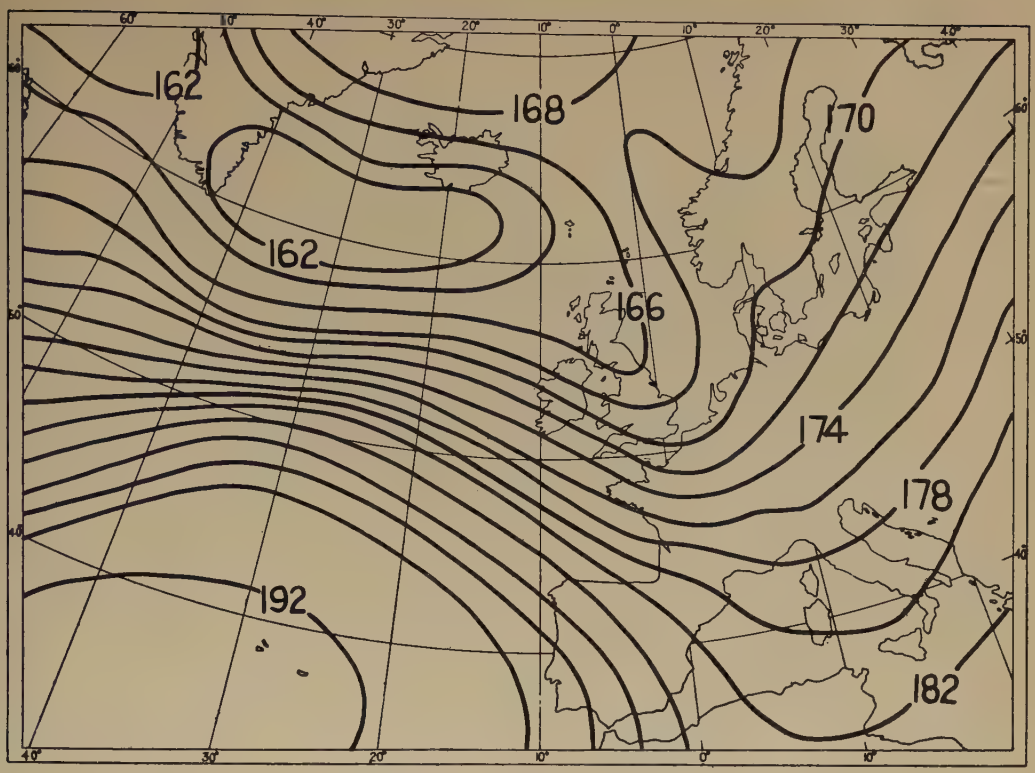

Fig. 2 (c). Actual 500-mb contour chart for 1500 GMT 31 Jan. 1952.

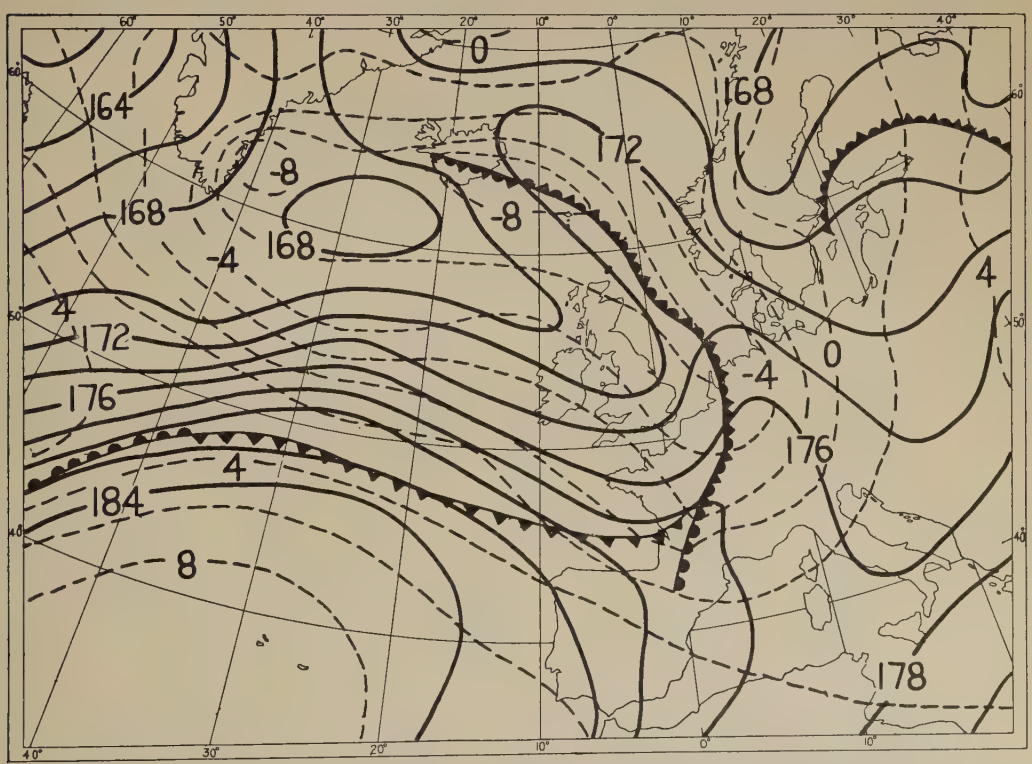

Fig. 2 (d). Actual 1,000-500 mb thickness and 1,000-mb contour charts for 1500 GMT 31 Jan. 1952.

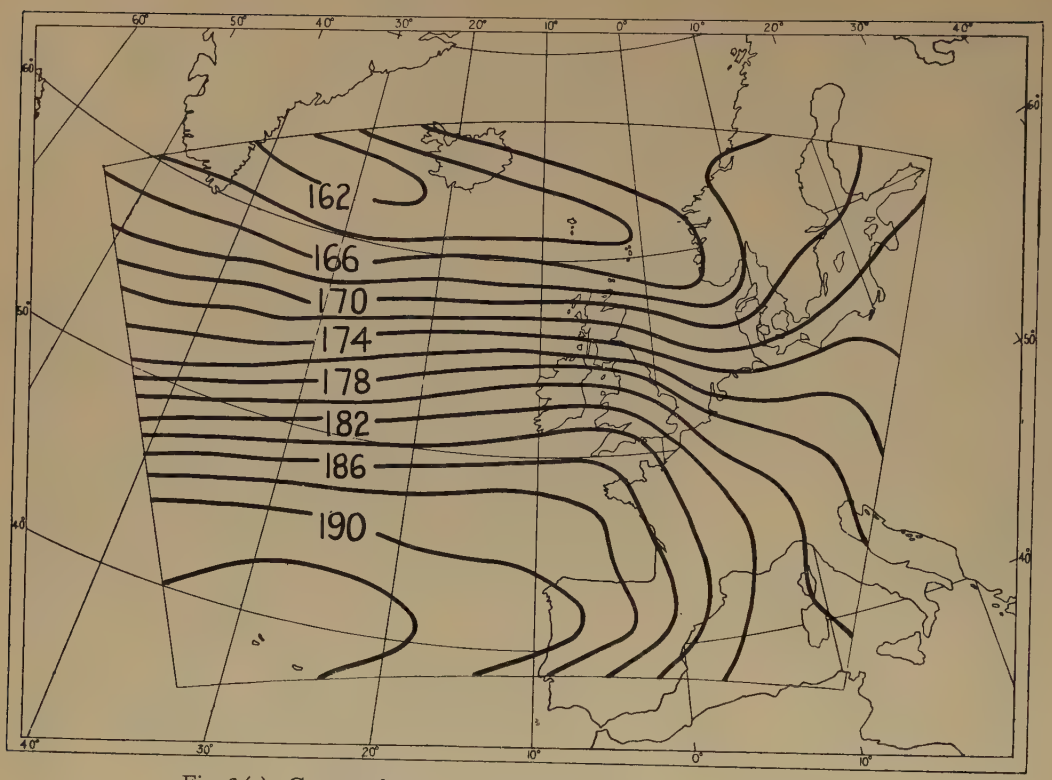

Fig. 2 (e). Computed 500-mb contour chart for 1500 GMT 31 Jan. 1952.

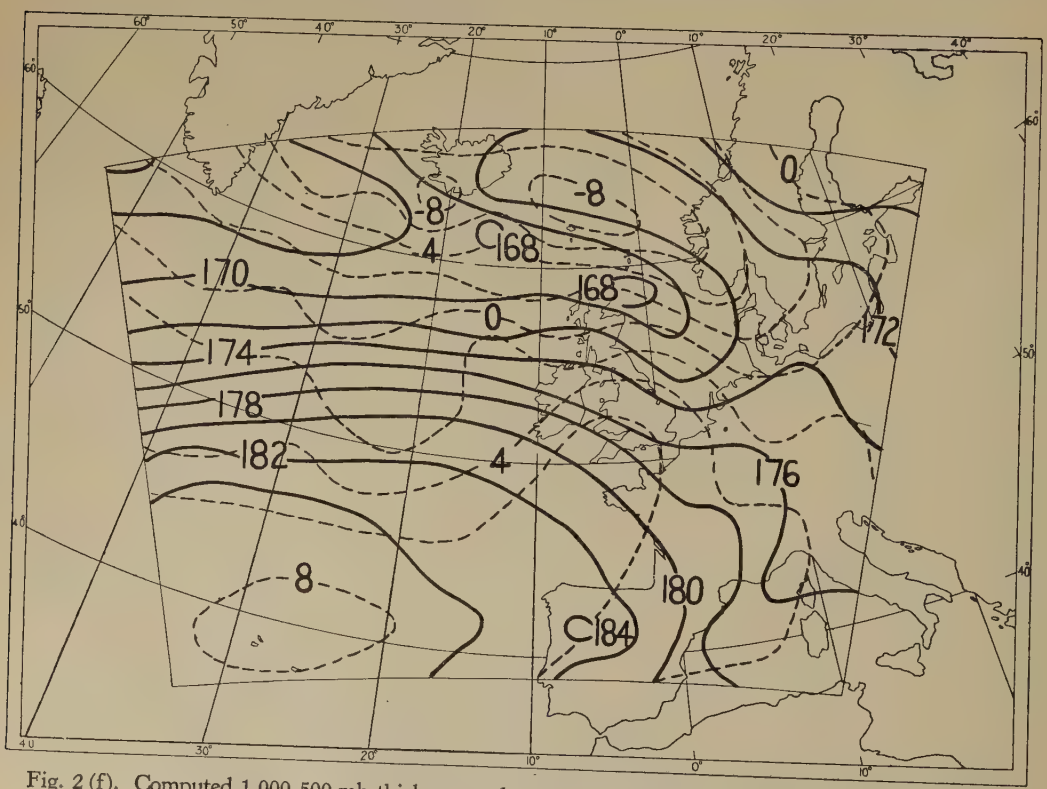

Fig. 2 (f). Computed 1,000-500 mb thickness and 1,000-mb contour charts for 1500 GMT 31 Jan. 1952.

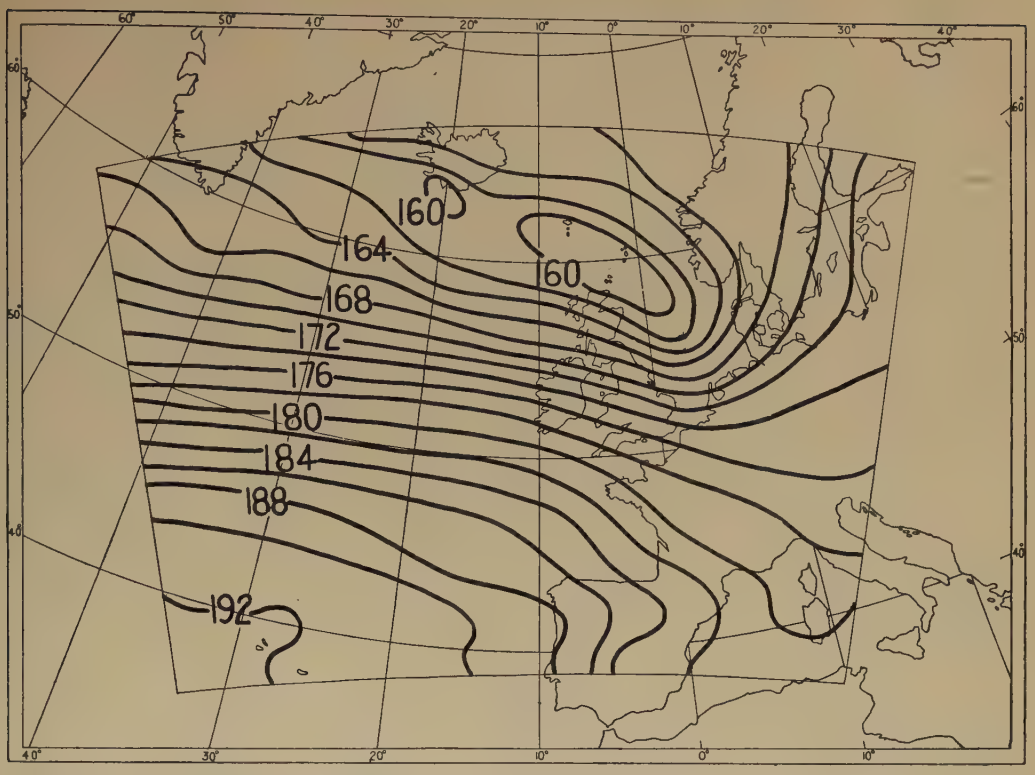

Fig. 2 (g). Computed 500-mb contour chart (using stream function) for 1500 GMT 31 Jan. 1952.

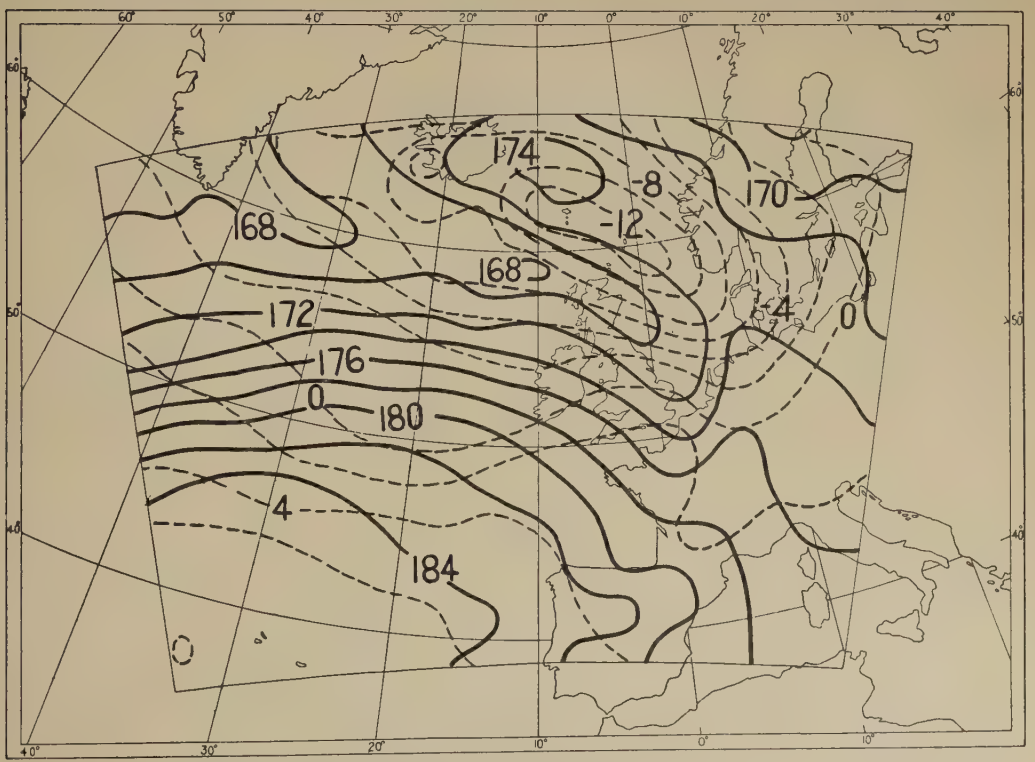

Fig. 2 (h). Computed 1,000-500 mb thickness and 1,000-mb contour charts (using stream function) for 1500 GMT 31 Jan. 1952.

5. Conclusions

This experiment has shown that it is quite practicable to carry out the solution of the Monge-Ampère equation on an electronic computing machine by using the iterative procedure described in § 2. The use of the stream-function technique extends the length of the computation from about $3\frac{1}{2}$ hr to 4 hr on the Ferranti Mark I electronic computer at Manchester University.

Apart from a systematic tendency to produce lower forecast values than actually occurred, owing to an initial reduction of the total anticyclonic geostrophic vorticity in the forecast area, there was no indication in any of the three situations examined that the use of the stream function made the computed forecast worse. In two situations there was little difference in the forecasts with and without the stream function. On the other occasion the computed forecasts using the stream function were better than the corresponding forecasts without the stream function.

It is impossible to decide on the basis of the three situations so far examined whether or not it would be desirable to extend the length of the computation by 10 per cent in order to make use of the stream function in the two-parameter model in the crude manner described in this paper. Nevertheless the authors believe that in cases of strong winds the technique described in this paper would often make a significant improvement in the forecast, whereas in other cases it would make little difference.

Acknowledgments

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Waves in the lee of an isolated hill

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SUMMARY

A two-dimensional ridge produces a system of lee waves parallel to it, in a suitable airstream. Synthesizing an isolated hill by superposition of ridges inclined at various angles but intersecting in a point, and representing this by an integral, the interference pattern of the lee-wave systems of the component ridges has been computed. An isolated hill produces a lee-wave pattern very similar to that produced by a ship moving across the surface of deep water. It is confined within a wedge-shaped region, the corner of the wedge being vertical and through the hill top. The half-angle of the wedge depends upon the airstream characteristics but is typically about 15°. The waves decrease in amplitude downstream like $x^{-\frac{1}{2}}$. If an oval hill lies across the airstream an appreciable lee-wave system will be produced but it is greatly reduced in amplitude if the major axis of the hill is turned through 30° or more from the normal to the airstream. The discussion is restricted, on account of its complexity, to airstreams composed of only two layers, in each of which the parameter l is independent of height.

1. The nature of the waves

Previous work has shown that a train of standing waves in the lee of a hill will only be found if there is a variation with height of the parameter l^2 of the airstream flowing over the hill. The simplest case to discuss is that in which the airstream consists of two layers in each of which l^2 is independent of height, with a discontinuity of l at the interface. When a train of lee waves is produced by a two-dimensional hill the amplitude of the waves does not decrease downstream if there is no friction, but since the disturbance of an isolated hill can spread out sideways in the manner of the wash of a ship on a water surface, it is to be expected that the wave train produced by an isolated hill will spread likewise. In this paper only the wave train, not the disturbance over the hill itself, is calculated.

It will be shown that the lee waves lie within a wedge-shaped region whose sharp edge is vertical and passes through the crest of the hill. The object of this investigation is to see how widely the angle of this wedge can vary, how the amplitude of the waves decreases in the downstream direction, how it compares with the amplitude of waves in the lee of a two-dimensional ridge, and how the amplitude varies across the wedge.

No results of a general nature can be deduced because of the large number of variable quantities determining the motion, and it is necessary to restrict the discussion and to illustrate the possibilities by particular examples. The simple two-layer case only is considered, and the values of l chosen for the examples are such that the wavelength of the lee waves is about 2π km. Similar but not identical results could be obtained for longer and shorter wavelengths. The notation is the same as in previous papers (Scorer 1949, 1953, 1956; the first two being referred to as I and II respectively).

2. Theory

The waves in the lee of the ridge whose height is given by

$$\zeta_1 = \frac{a^2 b}{b^2 + x^2} \qquad (2.1)$$

are (see II, Eq. (26) and Eq. (33))

$$L = -2\pi a^2 \frac{\zeta_{z,k}}{\partial \zeta_{1,k}/\partial k} e^{-kb} \sin kx \qquad (2.2)$$

where $2\pi/k$ is the wavelength of the lee-waves.

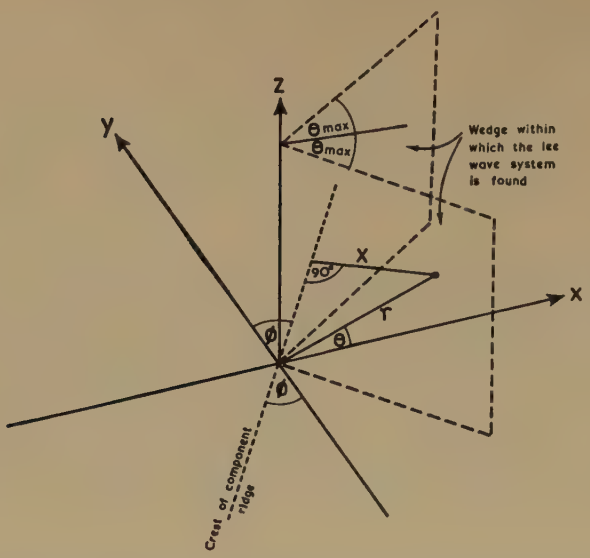

Figure 1. The system of coordinates used. The origin is under the centre of the hill and the wedge, of half-angle θ_{max}, within which the lee-wave system lies, is shown.

If the ridge is inclined at angle ϕ to the normal to the airstream (see Fig. 1) then k depends on ϕ, and x is replaced by X, where

$$X = r\cos(\theta + \phi) \quad . \quad . \quad . \quad . \quad . \quad (2.3)$$

and (r, θ) are horizontal polar coordinates.

An isolated hill whose height is defined (as in Scorer 1956) by

$$\zeta_1 = \int_{-\frac{\pi}{2}}^{\frac{\pi}{2}} \Phi \frac{a^2 b}{b^2 + X^2}\, d\phi \quad . \quad . \quad . \quad . \quad . \quad (2.4)$$

therefore produces a lee-wave system given by

$$L = -\int_{-\frac{\pi}{2}}^{\frac{\pi}{2}} \Phi\, 2\pi a^2 \frac{\zeta_{z,k}}{\partial\zeta_{1,k}/\partial k} e^{-kb} \sin kX\, d\phi \quad . \quad . \quad (2.5)$$

For a given (r, θ, z), kX changes rapidly as ϕ changes, while the other components of the integral are slowly changing functions of ϕ. Sin kX therefore oscillates rapidly over the range of integration and the principle of stationary phase can be used to evaluate the integral, giving

$$L = i\,\Phi\, 2\pi a^2 \frac{\zeta_{z,k}}{\partial\zeta_{1,k}/\partial k} e^{-kb} \left(\frac{2\pi}{|\partial^2(kX)/\partial\phi^2|}\right)^{\frac{1}{2}} \exp i\left(kX \pm \frac{\pi}{4}\right) \text{(real part)} \quad . \quad (2.6)$$

the sign in the exponent being that of $\partial^2(kX)/\partial\phi^2$, and the whole expression being evaluated for the value of ϕ satisfying

$$\frac{\partial}{\partial\phi} kX = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (2.7)$$

In Eq. (2.6) Φ determines the shape of the hill. If Φ is independent of ϕ the hill has circular contours, otherwise it will have its major and minor axes along the directions ϕ in which Φ is a maximum and minimum respectively.

$\zeta_{z,k}$ is a function of z and k only, and is the vertical displacement of the air at height z. Together with $\partial\zeta_{1,k}/\partial k$ it determines the amplitude of the waves and its variation with height. By II, Eq. (33)

$$\frac{\zeta_{z,k}}{\partial\zeta_{1,k}/\partial k} = \frac{U_1 w_{z,k}}{U_z \partial w_{1,k}/\partial k} \quad . \quad . \quad . \quad . \quad . \quad (2.8)$$

and in this paper we assume that U is independent of height so that $U_1 = U_z$. The results can be applied to other cases simply by applying a factor U_1/U_z to the amplitude of the waves.

$w_{z,k}$ satisfies

$$\partial^2 w/\partial z^2 + (l^2 - k^2)\, w = 0 \quad . \quad . \quad . \quad . \quad (2.9)$$

and in the case of an airstream consisting of two layers (denoted by suffixes i and s), the lower one being of depth h and the upper one extending to infinity upwards, we find that (See I, Eq. (16))

$$\nu_i \cot \nu_i h = -\mu_s \quad . \quad . \quad . \quad . \quad . \quad (2.10)$$

on the assumption that the vertical velocity is zero at the ground and dies away to nothing at great height. Eq. (2.10) determines the length of the lee waves for each angle ϕ. Here

$$\left.\begin{aligned} \nu_i &= +(l_i^2 - k^2)^{\frac{1}{2}}, \quad l_i^2 = g\beta_i/U_i^2 \cos^2\phi \\ \mu_s &= +(k^2 - l_s^2)^{\frac{1}{2}}, \quad l_s^2 = g\beta_s/U_s^2 \cos^2\phi \end{aligned}\right\} \quad . \quad . \quad (2.11)$$

Eq. (2.10), together with the definitions Eq. (2.11), determines k as a function of ϕ for a given set of values of l_i, l_s and h, i.e. for a given airstream. More strictly, l^2 contains also a term U''/U which varies differently from $g\,\beta/U^2 \cos^2\phi$ as ϕ varies. No very significant difference in the results would be obtained if this were included in the calculations, so it has been omitted for simplicity.

Differentiating Eq. (2.10) with respect to ϕ and substituting in Eq. (2.7) for $\partial k/\partial\phi$ we obtain

$$\tan(\theta + \phi) = \frac{\sin\phi}{k^2\cos\phi} \frac{-\mu_s l_i^2 (\cot \nu_i h - \nu_i h \operatorname{cosec}^2 \nu_i h) + \nu_i l_s^2}{-\mu_s (\cot \nu_i h - \nu_i h \operatorname{cosec}^2 \nu_i h) + \nu_i} \quad . \quad (2.12)$$

In the expression Eq. (2.6) the exponential shows that there is a wave pattern. The points on a wave crest (or trough, or node) at a given value of z lie on a line along which the exponent is constant, i.e.

$$kX = N = \text{const.} \quad . \quad . \quad . \quad . \quad . \quad (2.13)$$

By means of the two equations, (2.12), (2.13), for a given N, for each value of ϕ, r and θ can be determined. In the course of the calculation Eq. (2.10) has to be used to give k as a function of ϕ. If ϕ is allowed to vary over the range $(-\pi/2, \pi/2)$ a curve, giving the points of the wave crest, is obtained. For different values of N other crests and troughs can be determined. Such a pattern is shown at the bottom of Fig. 2. The amplitude at each point is obtained by evaluating the expression Eq. (2.6).

It is found that for the lower and upper layers, when l^2 is constant in each,

$$\left.\begin{aligned} w_{z,k}/(\partial w_{1,k}/\partial k) &= \frac{\nu_i}{k} \frac{1}{h + 1/\mu_s} \sin \nu_i (z + h), \quad -h \leqslant z \leqslant 0 \\ &= \frac{\nu_i}{k} \frac{\sin \nu_i h}{h + 1/\mu_s} e^{-\mu_s z}, \quad z \geqslant 0 \end{aligned}\right\} \quad . \quad (2.14)$$

the boundary between the two layers being taken at $z = 0$, and h being the depth of the lower layer. These formulae are obtained by taking the general solution of Eq. (2.9) with $l = l_i$, differentiating with respect to k, and putting z equal to $-h$ to obtain $\partial w_{1,k}/\partial k$. Eq. (2.10) is used to simplify the resulting expression.

3. The form of the wave pattern

Using ϕ as a variable we may draw out the pattern of the waves, the crests being given by $N = (2n + \frac{1}{4})\pi$, for instance. Eqs. (2.10), (2.12), and (2.13) then show that as ϕ increases from zero to somewhere around 30°, θ increases to a maximum, which according to the model airstream taken, may be anywhere between about 5° and 30° for practical cases; but for airstreams like those most commonly observed, the maximum value of θ is at about 12°. As ϕ increases from there onwards, θ slowly decreases. At the same time r increases from a minimum at $\phi = 0$ to a maximum at the same value of ϕ as gives the maximum of θ, and thereafter decreases. This means that there is no disturbance at all outside the wedge bounded by the maximum values of θ, and the form of the waves has a cusp at this boundary, as in the wash of a ship (see bottom of Fig. 2).

By Eqs. (2.7) and (2.3) we see that

$$\frac{\partial k}{\partial \phi} r \cos(\theta + \phi) - kr \sin(\theta + \phi) = 0$$

or

$$\frac{\partial k}{\partial \phi} = k \tan(\theta + \phi) \quad . \quad . \quad . \quad . \quad . \quad . \quad (3.1)$$

Also

$$\frac{\partial^2 (kX)}{\partial \phi^2} = r \cos(\theta + \phi) \left\{ \frac{\partial^2 k}{\partial \phi^2} - k - 2k \tan^2(\theta + \phi) \right\} . \quad . \quad (3.2)$$

But differentiating Eq. (3.1), on the assumption that $\partial\theta/\partial\phi = 0$ we obtain

$$\frac{\partial^2 k}{\partial \phi^2} = \frac{\partial k}{\partial \phi} \tan(\theta + \phi) + k \sec^2(\theta + \phi)$$

$$= k(1 + 2\tan^2(\theta + \phi)) \quad . \quad . \quad . \quad . \quad . \quad (3.3)$$

Substituting in Eq. (3.2) for $\partial^2 k/\partial\phi^2$ we find that

when $\partial\theta/\partial\phi = 0$, then $\partial^2 (kX)/\partial\phi^2 = 0$ . . . (3.4)

that is to say that at the cusp on the wave pattern the quantity $\partial^2 (kX)/\partial\phi^2$ changes sign, and $\pi/2$ is therefore subtracted from the exponent in Eq. (2.6). Near the cusp the formula Eq. (2.6) fails to evaluate the magnitude of the waves but since a crest and a trough meet there, and there is no disturbance for greater values of θ, we presume that the disturbance tends to zero as the cusp is approached.

Near the centre of the wave pattern, close to $\phi = 0$ and $\theta = 0$, the formula (2.6) gives a good estimate because $\partial^3 (kX)/\partial\phi^3$ is zero there. Sections showing the streamlines in the vertical planes $\theta = 0$ are therefore shown in Figs. 2 and 3. The decrease in amplitude of the waves in the downstream direction in the plane $\theta = 0$ is like $x^{-\frac{1}{2}}$, because r occurs in the right-hand side of Eq. (3.2), and outside the exponential in Eq. (2.6) distance downstream occurs only in the term $\partial^2 (kX)/\partial\phi^2$.

The full formula which must be used for $\partial^2 k/\partial\phi^2$ in Eq. (3.2) when $\partial^2 (kX)/\partial\phi^2$ is computed, is obtained by eliminating θ between Eqs. (2.12) and (3.1) and differentiating again with respect to ϕ. This is equivalent to differentiating Eq. (2.10) twice with respect to ϕ. In fact, it was found in all cases that $\partial^2 (kX)/\partial\phi^2$ varied very little up to about three-quarters of the way out to the cusp. Its variation as a function of ϕ is shown in Fig. 4, in which, where the continuous line is drawn, the formula Eq. (2.6) is assumed to give substantially the correct value of the integral Eq. (2.5). The dashed part of the curve corresponds to the value of $\partial^2 (kX)/\partial\phi^2$ in the formula Eq. (2.6) which would give the correct value of Eq. (2.5).

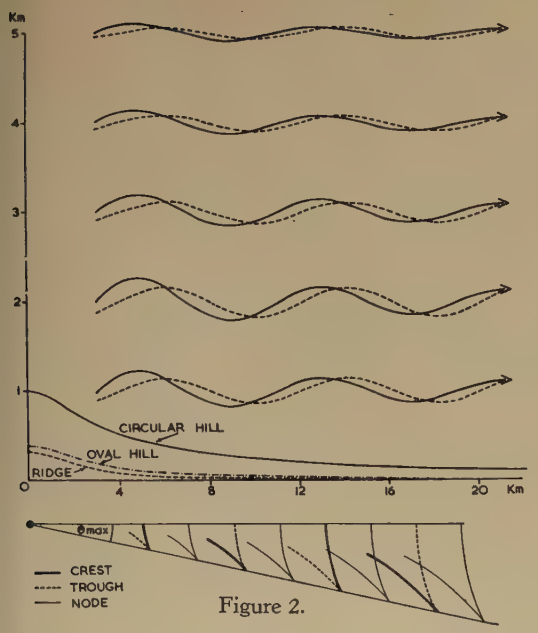

Figure 2.

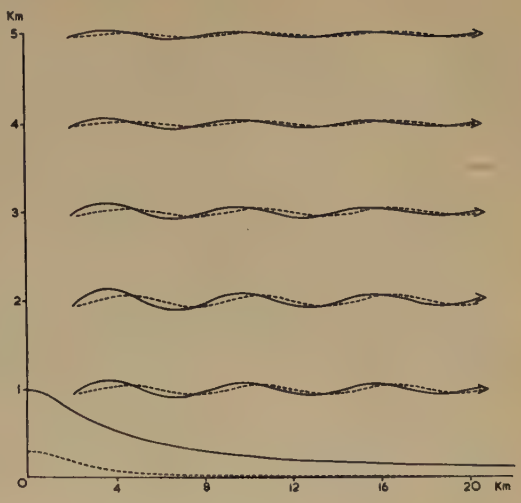

Figure 3.

Figure 2. The streamlines in the central plane $\theta = 0$, showing the lee-waves of a circular hill (continuous lines) and of a two-dimensional ridge (dashed lines) : the latter do not decrease in amplitude downstream. The mountain sections are shown below. The oval hill whose contours are shown in Fig. 6 produces the same lee-wave system in this central plane as the circular hill. This oval hill was derived using the function Φ shown in Fig. 7.

Below is shown the wave pattern in a horizontal surface for the isolated hills. It lies entirely within the wedge of angle θ_{max} through the hill centre.

The calculations were made for an airstream in which $h = 2$ km, $l_s^2 = 0{\cdot}5 \times 10^{-10}$, $l_i^2 = 1{\cdot}5 \times 10^{-10}$ and $\theta_{max} \simeq 12{\cdot}3°$.

The amplitudes of the mountain and the waves are arbitrary : the height scale applies strictly to the streamlines only.

Figure 3. The lee waves of a circular hill and a two-dimensional ridge for the case $h = 2$ km, $l_s^2 = 10^{-10}$, $l_i^2 = 2 \times 10^{-10}$, for which $\theta_{max} \simeq 7{\cdot}8°$.

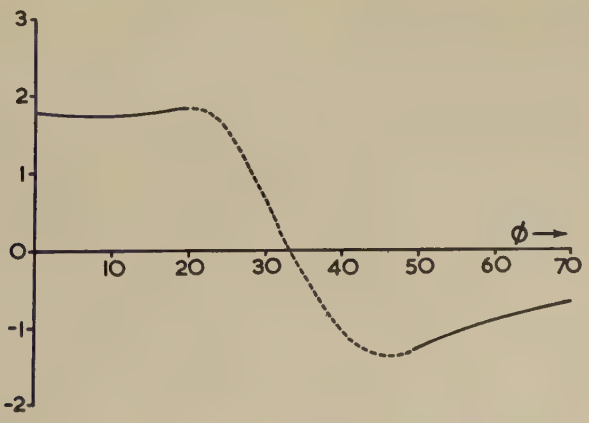

Figure 4. $[N/\{\partial^2 (kX)/\partial\phi^2\}]^{\frac{1}{2}}$ shown as a function of ϕ for the case of Fig. 2. The continuous parts of the curve show calculated values, with the sign appropriate to the phase of the wave. The dotted part gives estimated values of the function which, if inserted in Eq. (2·6), would give approximately the correct value for the integral (2.5).

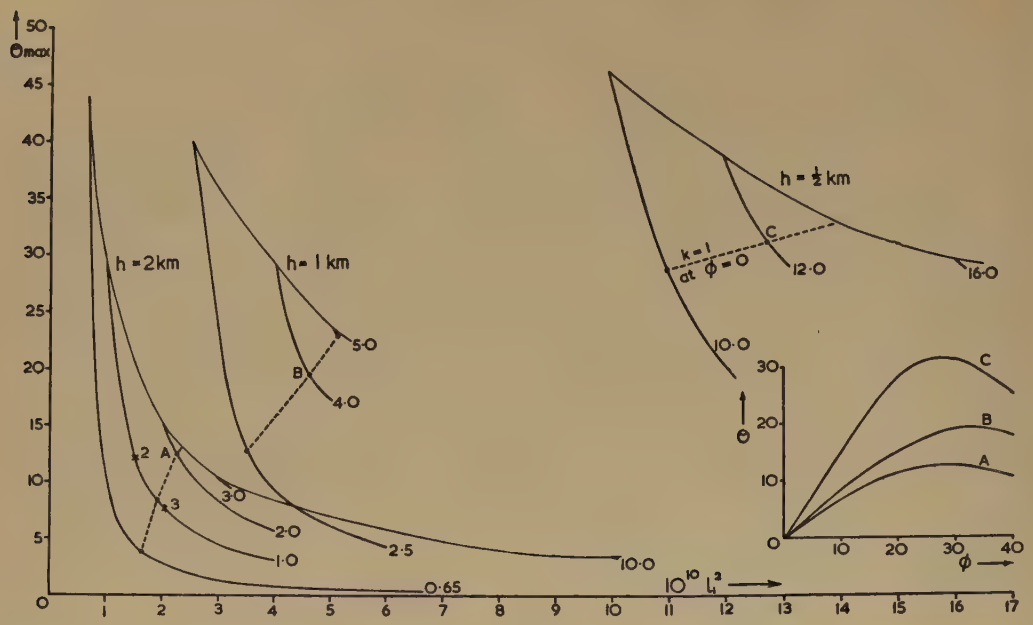

Figure 5. Values of θ_{max}, the angle of the wedge containing the lee-wave system, for variations in the airstream. Three groups of curves are shown, for $h = 2$ km, 1 km, $\frac{1}{2}$ km. In each of these the curves are drawn for those values of l_i^2 and l_s^2 for which there are lee waves at $\phi = 0$, viz. $l_i^2 - l_s^2 > \pi^2/4h^2$. The curves are for constant values of $l_i^2 - l_s^2$, and 10^{10} $(l_i^2 - l_s^2)$ is written against each curve. The abscissa is 10^{10} l_i^2.

The dotted lines show the airstreams for which $k = 1$ at $\phi = 0$, and the two crosses represent the airstreams of Figs. 2 and 3.

Inset is a diagram showing how θ varies as a function of ϕ. It is seen that θ_{max} corresponds very nearly to $\phi = 30°$ as it does also in Fig. 10. The curves in this figure were all calculated for $\phi = 30°$, and it was assumed that θ_{max} was very close to the value of θ thus obtained. The inset curves correspond to the points A, B, C on the main diagram.

4. The width of the wave pattern

In order to draw comparisons between different airstreams we concentrated mainly on cases in which the wavelength along $\theta = 0$ was about 2π km. (i.e. $k \simeq 1$ when $\phi = 0$). For airstreams of greatly differing character we found that θ reached a maximum when ϕ was about 30° (see Fig. 5), and so for simplicity θ was calculated in all cases for $\phi = 30°$ and assumed to have a maximum there. This gives the angle of the wedge containing the wave system. In Fig. 5 this angle, θ_{max}, is shown as a function of h, the depth of the lower layer, and $l_i^2 - l_s^2$, which represents the difference in stability of the two layers.

It can be seen that for airstreams for which $k = 1$ at $\phi = 0$, the angle of the wedge increases as h decreases, but that in order for this to happen $l_i^2 - l_s^2$ must at the same time increase enormously. Thus, to obtain a wide angle the airstream must consist of a very shallow very stable lower layer. On the other hand, for given values of h and $l_i^2 - l_s^2$, as l_i^2 varies θ_{max} and k (at $\phi = 0$) vary in such a way that as the stability of the lower layer increases θ_{max} decreases. k follows l_i (being very roughly midway between l_i and l_s), so when the stability is very high the wavelength is very short.

As can be seen in Fig. 5, l_i^2 has a minimum value determined by the requirement

$$l_i^2 - l_s^2 > \pi^2/4h^2 \quad . \quad . \quad . \quad . \quad (4.1)$$

which must be satisfied if there are to be any waves at all.

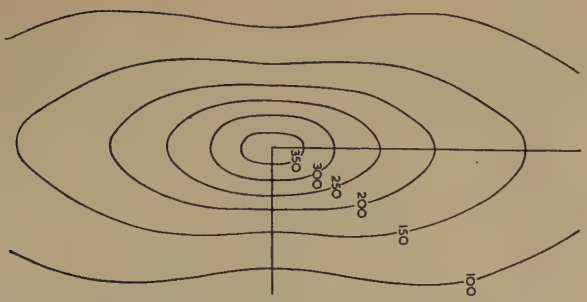

Figure 6. The contours of a solitary hill for which, if the wind flows along the minor axis, the lee-wave system in the plane downstream from the crest is the same as for the circular hill whose section is shown in Figs. 2 and 3.

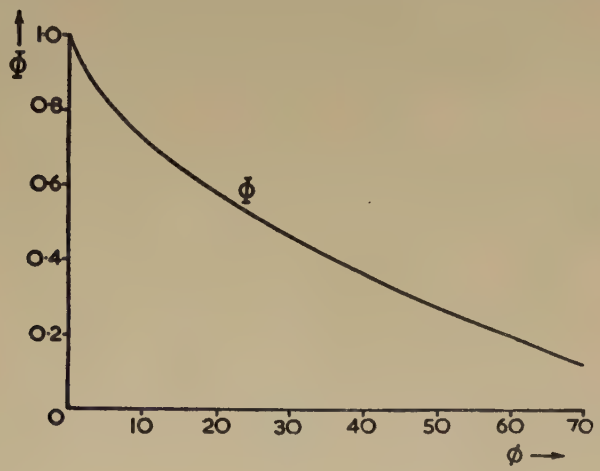

Figure 7. The function Φ used to synthesize the oval hill of Fig. 6.

5. The amplitude of the waves

In Figs. 2 and 3 we have drawn streamlines of particles in the central plane $\theta = 0$. These are compared with the paths of particles in the same airstream but in the lee of a two-dimensional ridge whose section is shown in a dashed line.

The solitary hill is about three times as high as the ridge but from about the second wave onwards produces a smaller disturbance.

The section drawn is of a circular hill. A hill of any section for which $\Phi = 1$ at $\phi = 0$ would produce the same lee-wave pattern in the central plane $\theta = 0$; for example, the hill whose contours are shown in Fig. 6 and whose section appears in Fig. 2, would do this, but the amplitude would be reduced by the factor Φ for other values of θ away from the central plane.

For this particular hill Φ is shown as a function of ϕ in Fig. 7. If this hill, instead of lying directly across the airstream, were inclined at 30° then Φ would be a maximum where the wave amplitude was zero, namely at the cusp.

By noting that $\phi \simeq 30°$ at the cusp of the wave pattern we can estimate roughly how the amplitude of the wave varies over the pattern by multiplying by the value of Φ corresponding to each point of the pattern. Similarly, from the graphs of Fig. 8, it can be seen how the remaining component of the formula (2.6) varies with ϕ. As we have already seen the amplitude falls to zero at the cusp, and beyond $\phi = 40°$ the quantities

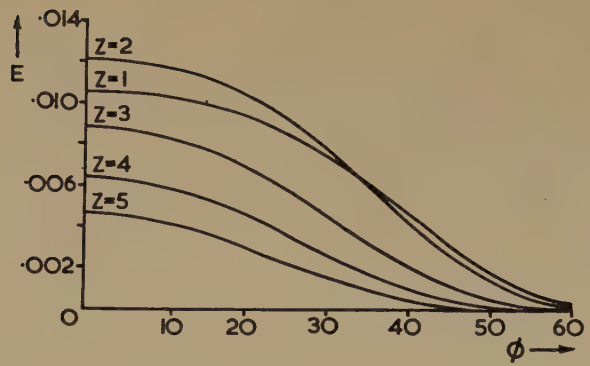

Figure 8. Values of $e^{-kb}\,\zeta_{z,k}/(\partial\zeta_{1,k}/\partial k)$, denoted by E, as a function of ϕ, for five different values of z. For ϕ substantially greater than 30°, E is small in comparison with its value at $\phi = 0$, which shows that the part of the wave shown in the bottom of Fig. 2 that does not lie across the central line is of small amplitude.

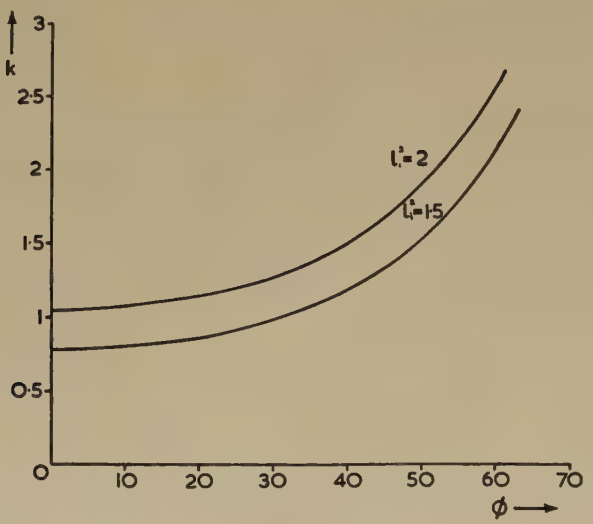

Figure 9. k shown as a function of ϕ for the two airstreams whose wave patterns are shown in Figs. 2 and 3.

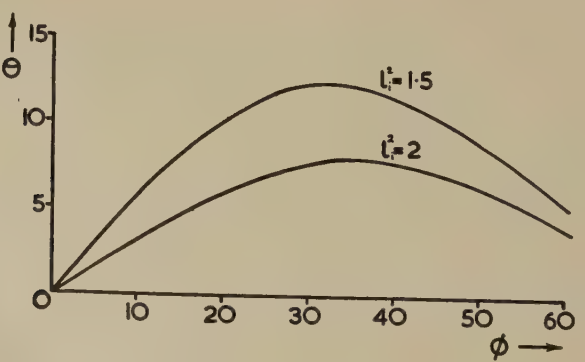

Figure 10. θ shown as a function of ϕ for the two airstreams of Figs. 2 and 3.

$e^{-kb}\zeta_{z,k}/(\partial\zeta_{1,k}/\partial k)$ and $(\partial^2(kX)/\partial\phi^2)^{-\frac{1}{2}}$ are small compared with their values at $\phi = \theta = 0$; thus the waves beyond the cusp are of very small amplitude and are unlikely to be observable except in specially favourable circumstances.

In Figs. 9 and 10 we show how k and θ vary as functions of ϕ for the cases shown in Figs. 2 and 3.

6. Conclusions

The values of the parameters h, l_i, and l_s have been allowed to vary widely enough in this investigation to reveal that airstreams in which lee waves can be set up do not behave with any major qualitative differences when excited into waves by flow over a ridge or over an isolated hill. The lee waves are of small amplitude compared with those produced by a long ridge of similar section except directly downwind of the hill crest and within one or two wavelengths of it. The disturbance is generally confined within a wedge of semi-angle about 15° to 20°, and often less than this. The only part of the disturbance likely to be observable is where the waves lie across the line downwind of the hill crest; the other part of the pattern is of insignificant amplitude.

The nature of the flow near to the hill crest itself is a form similar to that for a uniform stream. The vertical displacement has been computed previously (Scorer 1956) and no new points of interest would emerge if it were calculated for the case of the two-layer model; the addition of the lee waves computed here is the only important difference.

References

Scorer, R. S. 1949 *Quart. J. R. Met. Soc.*, **75**, p. 41.
1953 *Ibid.*, **79**, p. 70.
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551.594.13 : 551.594.12

Space and time variations of charged nuclei and electrical conductivity of the atmosphere

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Summary

The time variations of electrical conductivity and charged nucleus concentration have been investigated in fair weather in the altitude range 700 to 15,000 ft above the surface. In the exchange layer regular variations of nucleus concentration with time are observed from around sunrise to early afternoon. Analysis of the meteorological data obtained simultaneously shows that this variation is due primarily to the daily turbulent cycle. During periods of low advection, regular variations of nucleus concentration with time from early afternoon to the following sunrise are also observed in this region, and are shown to be the result of nucleus coagulation and subsidence. The time variations at night are more complicated during periods of intense advection. Above the exchange layer variations in nucleus content with time are not found to be regular. The variations in electrical conductivity with time are the inverse of the changes in nucleus concentration. The observed relation between conductivity and nucleus concentration is in good agreement with the theory of small ion balance, using Thomson's theory for small ion volume recombination and Bricard's theory for collisions between small ions and nuclei. An analysis of observed daily variations in the magnitude of the horizontal fluctuations of these variables is also given.

1. Introduction

The results of a systematic investigation of the variations with time in the vertical distribution of charged nuclei (large ions) and the electrical conductivity of the atmosphere are presented. The experiments were carried out in a B-17 aircraft with instruments for measuring and recording the concentration and mobility distribution of charged particles with mobility between 7×10^{-1} and 2×10^{-4} cm^2/sec-volt. Applying the Stokes-Millikan-Cunningham relation (Loeb 1947) this corresponds to particles with radii between 8×10^{-8} and 6×10^{-6} cm, assuming they are singly charged. The measurement, therefore, includes the charged component of Aitken nuclei. Since the size distribution is measured, the total nucleus concentration can be easily calculated from Bricard's theory (1949) for the collision process between small ions and nuclei. Electrical conductivity, temperature, pressure, humidity and airspeed were also recorded. The apparatus and method of measurement have been described in an earlier paper (Sagalyn and Faucher 1954).

The discussion presented here is limited primarily to results obtained in fair weather in the altitude range 700 to 15,000 ft above the surface. These experiments are a continuation of experiments described in an earlier paper (Sagalyn and Faucher 1954) where the results of an investigation of the vertical distribution of large ions and electrical conductivity during the daylight hours were presented. The results of approximately 50 flights, although showing considerable variation in the profile and magnitude of the charged nucleus concentration and conductivity at a given level, also showed certain general characteristics which could be understood in terms of known meteorological phenomena. For example, these flights showed that in the exchange or friction layer, varying in depth from 1,000 to 10,000 ft above the surface, the concentration and distribution of nuclei were determined largely by the intensity of atmospheric turbulence. In this region, collisions between small ions and nuclei had a dominant influence on the electrical conductivity of the atmosphere. At the top of this layer there was a sharp decrease in the nucleus concentration, which generally resulted in an abrupt termination

of their influence on the electrical properties of the atmosphere. Knowledge of the time variations of electrical conductivity and nucleus concentration is of importance in understanding the factors controlling the atmospheric electric current systems over continents, and also the introduction, motion and disappearance of nuclei in the atmosphere.

2. Method

Approximately 25 series of flights were carried out in fair weather. Each series consisted of 3 to 7 flights over an 8 to 24-hr period. The average time interval between flights was 4 hr. All measurements in a given series were made in the same air mass. Most of the measurements were carried out over a fixed flight path, 25 mi long, in southern New Hampshire. A few were carried out in Texas and in the vicinity of Mt. Palomar in southern California. On each flight constant altitude measurements were made between 700 and 15,000 ft above the surface (1,000 to 15,300 ft above sea level) at altitude intervals of 1,000 ft up to 10,000 ft and at intervals of 2,000 ft above this level. Measurements were carried out in all seasons of the year. Days on which there were no unbroken cloud layers in the immediate vicinity of the flight path are referred to as fair-weather days. The mean values of the recorded parameters at a given level are used in plotting the vertical distributions shown in Figs. 2, 3, 4 and 6. Altitudes above sea level are given in these figures. The nature of the horizontal variations in space and time will be discussed later.

3. Charged nuclei results

From shortly after sunrise to early afternoon, all fair-weather flights show a regular variation with time of the charged nucleus concentration, from the lower limit of measurement up to the level where turbulent and convective mixing ceases. This variation consists of a continuous increase with time in the charged nucleus concentration at a given level, reaching a maximum at approximately 15 hr LST, as shown in Fig. 1 (a). The

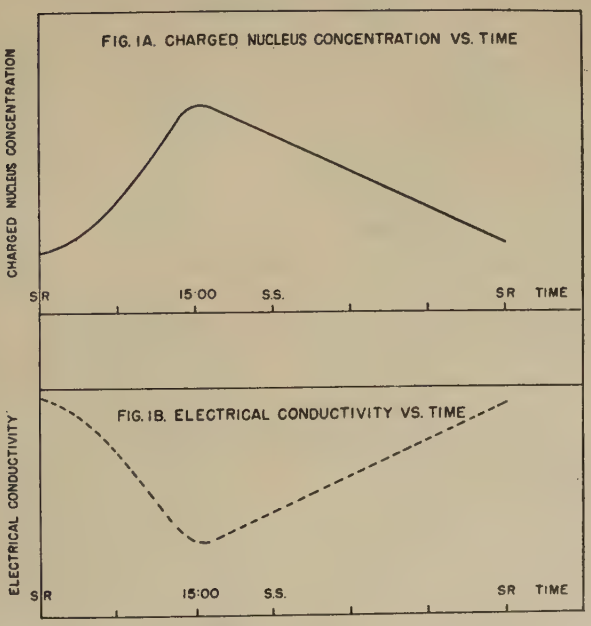


Figure 1. Time variation of charged nucleus concentration and electrical conductivity at constant altitude in the exchange layer during periods of low advection.

height of the exchange layer generally reaches a maximum at the time of maximum nucleus concentration. Illustrations of this type of variation are shown in the results of 20 August 1953 and 13, 14 May 1954 in Figs. 2 (a) and 3 (a).

The late afternoon and night results are more complicated. Approximately 65 per cent of the results show an essentially continuous decrease with time at a given level until shortly after sunrise (Fig. 1 (a)). The magnitude of the decrease varies considerably on the nights examined and is found to be dependent on the value of the charged nucleus concentration. The percentage decrease over a given period is greater for days with greater nucleus content. An example of this type of variation during the late afternoon and night hours is shown in the results of 13, 14 May 1954 in Fig. 3 (a).

35 per cent of the results show a different type of variation between 15 hr and sunrise, which usually consists of an increase in large ion content for at least several hours, sometimes at all altitudes in the exchange layer, sometimes in only a limited altitude range. The time of increase is not found to be associated with a definite local time on the days investigated. In Fig. 4 (a), showing the results of 21, 22 January 1954, an example of this type of variation is given.

Above the exchange layer the variations in large ion concentration with time were not found to be regular. Note the variations in large ion concentration with time above the exchange layer in Figs. 2 (a) to 4 (a).

The maximum and minimum daily variations of the charged nucleus concentration measured during the period of these experiments, in and above the exchange layer, are given in Table 1.

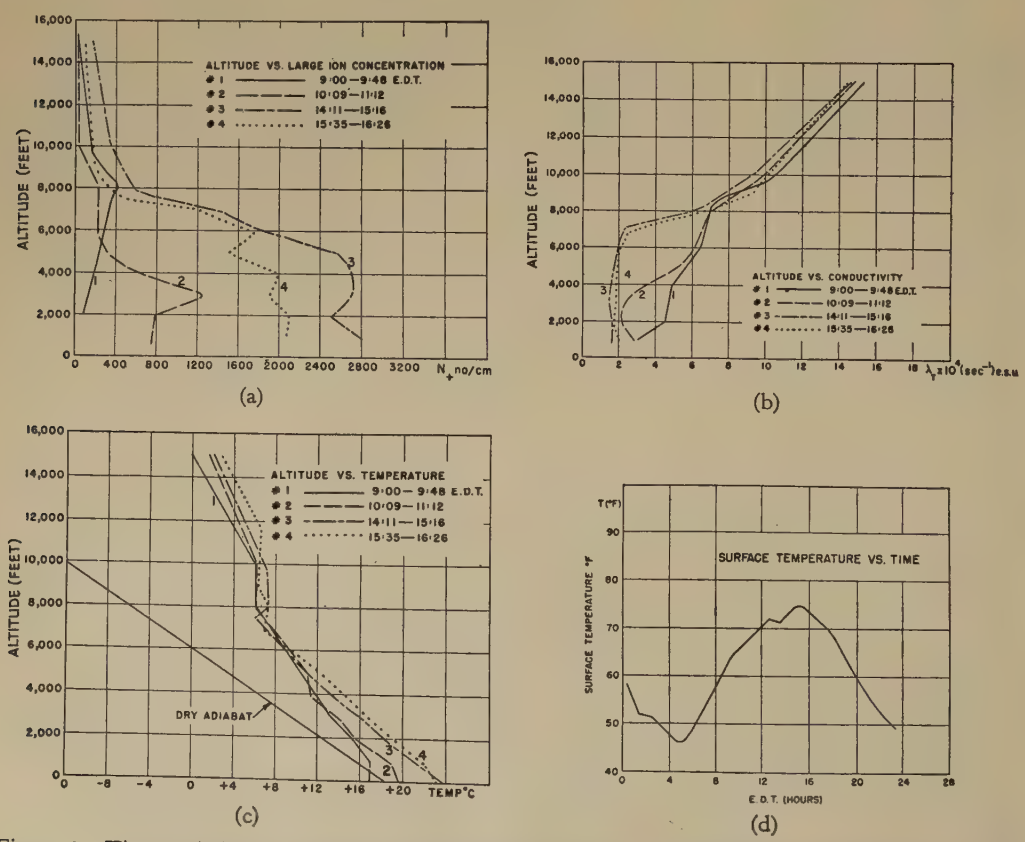


Figure 2. Time variation of charged nucleus concentration (N_+), total electrical conductivity (λ_T) and temperature (T) in southern New Hampshire, U.S.A., 20 Aug. 1953.

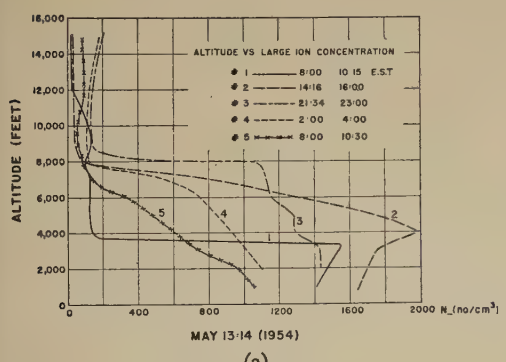
TABLE 1. MAXIMUM AND MINIMUM DAILY CHANGE IN CHARGED NUCLEUS CONCENTRATION AND ELECTRICAL CONDUCTIVITY MEASURED AT VARIOUS ALTITUDES

Altitude (ft)	$\Delta N_{\pm}$ (cm^{-3}) minimum	$\Delta N_{\pm}$ (cm^{-3}) maximum	$\Delta\lambda_T$ (sec^{-1} esu $\times 10^4$) minimum	$\Delta\lambda_T$ (sec^{-1} esu $\times 10^4$) maximum
		In exchange layer		
1,000	1,000	7,100	1·5	2·6
2,000	880	4,300	1·0	4·3
3,000	350	3,200	1·1	3·8
4,000	250	2,900	0·85	4·0
5,000	280	2,800	0·9	4·4
6,000	300			5·4
		Above exchange layer		
5,000	110		0·7	
6,000	95		0·7	
7,000	75		0·4	
8,000	90	300	1·0	3·6
9,000	80	350	0·9	4·1
10,000	125	640	1·2	4·4
12,000	120	500	1·3	5·3
15,000	210	450	2·0	4·4

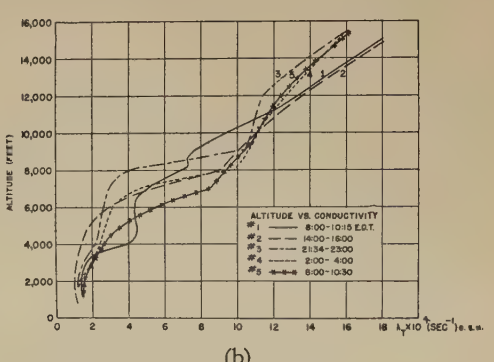
4. DISCUSSION OF RESULTS OF SECTION 3

(a) *Sunrise to 15 hr*

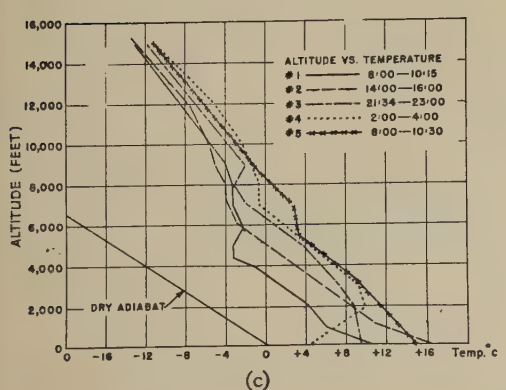
Study of the distribution of water vapour and temperature measured simultaneously in flight shows that the variation of the charged nucleus concentration with time in the exchange layer, from around sunrise to early afternoon, is due primarily to the daily



(a)



(b)



(c)

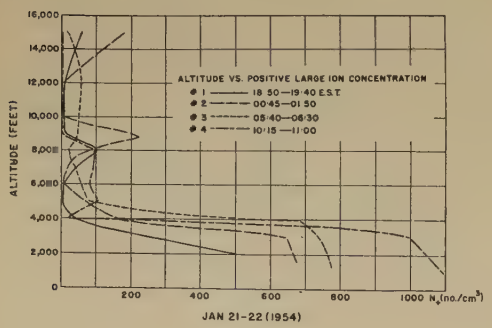
Figure 3. Time variation of charged nucleus concentration (N_-), total electrical conductivity (λ_T) and temperature (T) in southern New Hampshire, U.S.A., 13-14 May 1954.

turbulent cycle. Analysis of the results shows that the daily variations in large ion concentration at a given level follow closely the daily variation in the temperature gradient and also follow the variation with time of temperature and wind velocity at the surface (Brunt 1952). This is clearly illustrated, for example, in the results of 20 August in Figs. 2 (a) and 2 (c), which show that the temperature gradient becomes less stable between flights 1 (0900 hr) and 2 (1009 hr), up to 4,000 ft, corresponding to a considerable increase in nuclei in the same altitude range. On flight 3 at 1411 hr we see that the time of maximum nucleus concentration corresponds to the time of maximum temperature gradient. These features are also illustrated in the time variations of 13 May in Figs. 3 (a) and 3 (c).

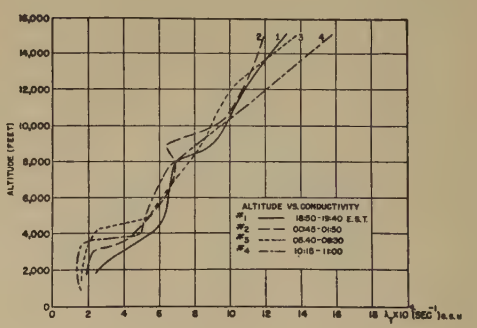
A quantitative check on the theory of mixing by atmospheric turbulence cannot be carried out directly because the austausch coefficient, as a function of time and height for a non-adiabatic atmosphere, is not known with sufficient accuracy. However, the equation of continuity is now being applied to these results to obtain information about the height variations of the austausch coefficient with time and with varying meteorological conditions.

(*b*) *15 hr to following sunrise – regular variation*

It should be noted that the time of the initial decrease in nuclei at approximately 15 hr, observed on 65 per cent of the series, also corresponds to the initial decrease in temperature and change to a more stable temperature gradient. The effect is most intense in the immediate vicinity of the surface. The surface wind data also show that this corresponds approximately to the time of initial decrease of wind velocity. The initial decrease in the nucleus count thus occurs when the rate of transport of nuclei and other physical properties from the surface is reduced.



(a)



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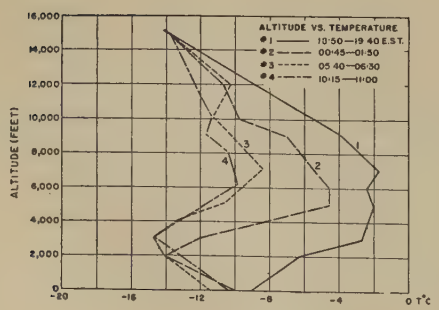

(c)

Figure 4. Time variation of charged nucleus concentration (N_+), total electrical conductivity (λ_T) and temperature (T) in southern New Hampshire, U.S.A., 21-22 Jan. 1954.

Several possible contributing factors were considered in attempting to explain this decrease in charged nucleus concentration with time, such as diffusion through the top of the exchange layer, advection, fall due to gravity, movement due to the electric field of the atmosphere, nucleus coagulation and subsidence. These processes will be considered in order :

(i) As indicated in the results for individual series, there was no experimental evidence of diffusion through the top of the exchange layer during the late afternoon and night hours.

(ii) Air-mass trajectories were determined from surface and upper-air weather maps up to 48 hr before the time of measurement. The results showed that during the period of these experiments there were variations in direction of approach of the air of over 180°; only air masses coming directly from the Atlantic Ocean were excluded. This indicates that changes in nucleus concentration with time due to advection should not be systematic.

(iii) Application of the modified Stokes relation (Millikan 1947) showed that the fall due to gravity of particles of radius $2{\cdot}3 \times 10^{-6}$ cm is negligible over a 15-hr period. This is the average radius of maximum nucleus concentration obtained from our measurements of the mobility distribution of atmospheric ions.

(iv) Calculations show that the effect of the vertical movement of these charged nuclei in the atmospheric electric field over a 15-hr period is also negligible.

(v) The theoretical expression for the coagulation process for uniformly sized spherical particles, derived by Smoluchowski (1918), can be written :

$$-dZ/dt = \gamma Z^2 \qquad (1)$$

where Z is the total nucleus concentration and t is time. When the radius of the particles is less than or approximately equal to the mean free path of the molecules of the gas the coagulation coefficient is expressed :

$$\gamma = \frac{4RT}{3N_M \eta}(1 + AL/a) \qquad (2)$$

where R is the gas constant, T the absolute temperature, N_M is Avogadro's number, η the viscosity of the gas, A is a constant, L the mean free path of the gas molecules and a is the particle radius.

Eq. 1 was solved for Z as a function of time using a value of the coagulation coefficient $\gamma = 1{\cdot}6 \times 10^{-9}$ cm^3 sec^{-1}, computed from Eq. 2 for particles with mean radius $2{\cdot}3 \times 10^{-6}$ cm at 20°C and 760 mm Hg. The variation in γ due to density variations is negligible between the surface and 4,000 ft, the altitude region we will consider. In Fig. 5 the solid lines represent the computed variation of Z with time over the time interval in which coagulation is being considered as the most important process modifying the concentration of nuclei. These show that an initial concentration of 9,600 per cm^3 is reduced by coagulation 45 per cent in 14 hr, while an initial nucleus concentration of 1,200 per cm^3 is reduced only 8 per cent over the same period. Examples of nucleus variations with time obtained experimentally are indicated by the dashed curves in the same figure. Bricard's theory (1949) was applied to obtain the total nucleus concentration from our measurements of charged nuclei. From this theory the ratio of uncharged to charged nuclei of either sign $N_0/N_\pm$, was calculated for particles with mean radius $2{\cdot}3 \times 10^{-6}$ cm. Assuming that the concentrations of positive and negative large ions are approximately equal, $Z = N_\pm(2 + N_0/N_\pm)$. The value of the ratio $N_0/N_\pm$ computed for particles of radius $2{\cdot}3 \times 10^{-6}$ cm is in good agreement with that obtained from the experiment results of Nolan and Kennan (1949) relating radius and the ratio of total to uncharged nuclei.

The influence of particle size distribution and the variation of the coagulation coefficient with varying mean radius as coagulation proceeds have not been considered in these computations. Assuming that the product Za^3 is approximately constant in the range of nucleus concentrations normally found in the atmosphere, the variation in radius will not have a significant effect on the coagulation rate over a 14-hr period. The analysis of the effect of heterogeneity by Whytlaw-Gray and Patterson (1932) for various size ranges indicates that heterogeneity has a small effect on the coagulation rate for atmospheric nuclei. These two corrections will also tend to cancel one another.

Away from the vicinity of the upper boundary of the exchange layer the experimental results, as illustrated in Fig. 5, generally show a decrease of nucleus count with time slightly greater than would be expected from coagulation alone. However, taking into account the limitations of the assumptions made in carrying out these calculations and the possibility of inhomogeneities in the air mass, the results show that coagulation is often the most important mechanism reducing the nucleus concentration during the night hours. The rate of decrease of nuclei with time in the vicinity of the upper boundary of the exchange layer is usually much greater than that given by the coagulation theory, and will be discussed later.

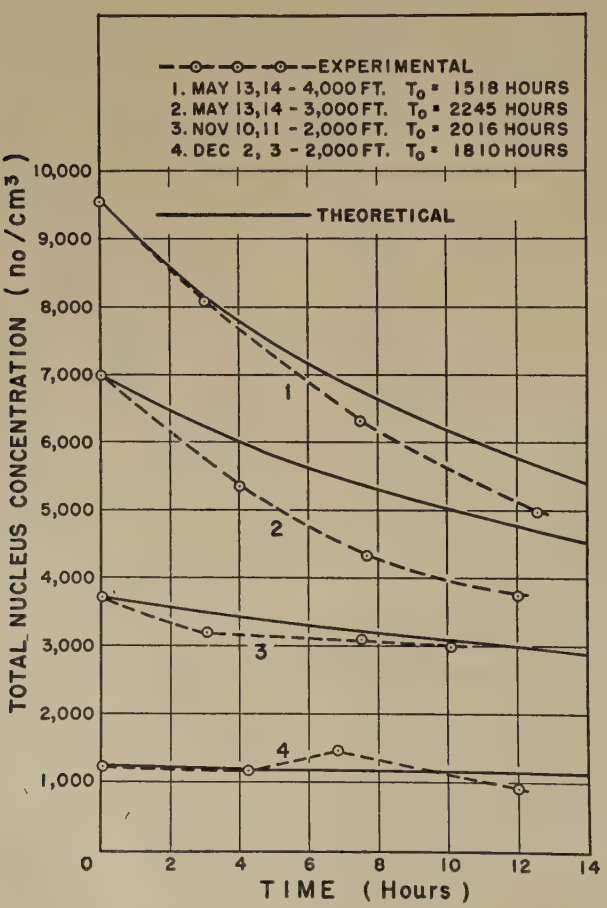

Figure 5. Theoretical and experimental variation of nucleus concentration with time. The theoretical values were computed assuming Smoluchowski's coagulation law.

(vi) The surface and upper-air weather maps and the wind magnitude and direction as a function of height, obtained from local balloon soundings, were also examined to determine to what extent changes in the temperature and water-vapour profiles during each series of measurements were due to advection, subsidence or lifting. After taking into account changes in temperature and humidity due to advection, this examination showed that subsidence occurred on approximately 85 per cent of the nights where regular variations in nucleus concentration with time were observed. This result is not surprising, since fair-weather measurements are prejudiced toward high-pressure areas. The sinking of atmospheric layers has the greatest effect on the nucleus distribution in the vicinity of the upper boundary of the exchange layer. During the period covered by these experiments the height of the exchange layer was lowered by subsidence, at varying rates, by amounts from a few hundred to 2,500 ft. The influence of subsidence is most easily seen on nights when the nucleus concentration is low so that loss due to coagulation is small. Consider, for example, the results of Nov. 10 and 11 1953, shown in Fig. 6. Examination of the upper-air maps showed that between flights 1 and 2, the changes in temperature between 5,000 and 9,000 ft in the vicinity of the top of the exchange layer were due to subsidence, which produces warming and drying as indicated in the temperature and humidity profiles of Fig. 6 (c) and (d) and a lowering of the exchange layer by approximately 2,000 ft. Between flight 2 at 0025 hr and flight 3 at 0345 hr, subsidence is negligible; and also there is essentially no change in the nucleus distribution in the exchange layer during this period. The influence of subsidence can also be seen in the results of 13 and 14 May (Figs. 3 (a), (b), (c)) between 21 hr and 08 hr. The very large decrease in nucleus concentration with time at altitudes in the vicinity of the upper

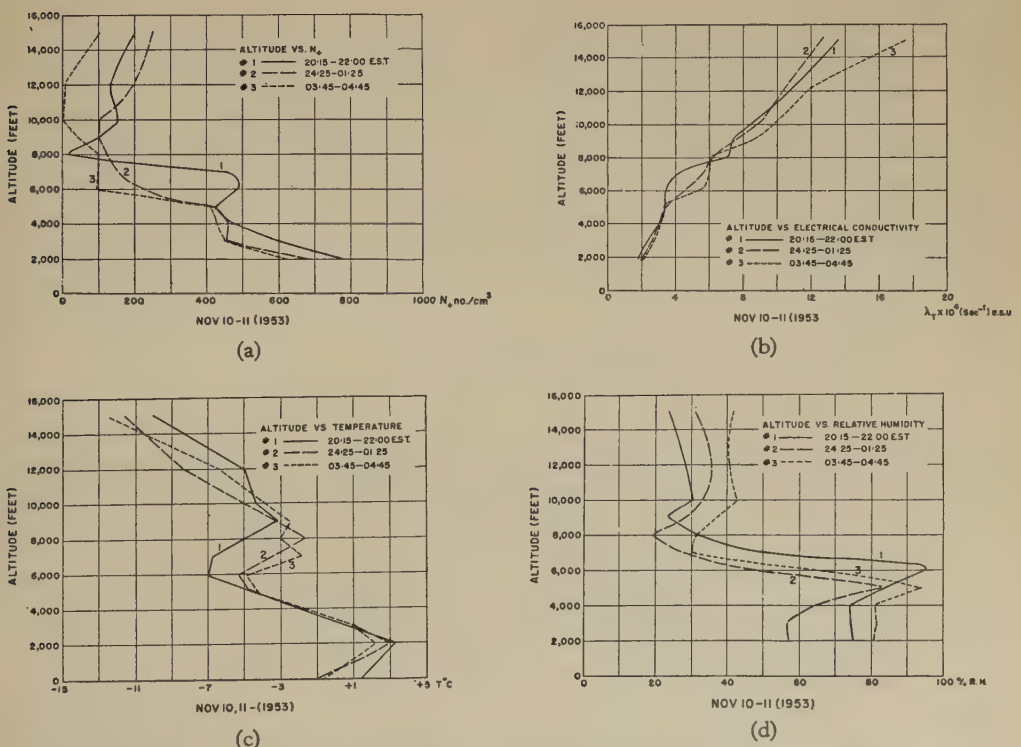

Figure 6. Time variation of charged nucleus concentration (N_+), total electrical conductivity (λ_T), temperature (T) and relative humidity (R.H.) in southern New Hampshire, U.S.A., 10-11 Nov. 1953.

boundary of the exchange layer mentioned in the discussion of coagulation is, therefore, largely due to the sinking of this layer. Further, since in general the nucleus concentration decreases with increasing height, subsidence is also effective at all altitudes in the exchange layer in producing a rate of decrease greater than would be expected from coagulation alone.

The analysis shows, therefore, that the decrease of nucleus concentration with time in the exchange layer, observed on 65 per cent of the night flights, is largely the result of coagulation and subsidence; the relative importance of each at a given altitude during a series of flights being dependent on the pressure system in the immediate vicinity of the flight path and upon the magnitude of the nucleus concentration.

(c) *15 hr to following sunrise – irregular variation*

Examination of the available meteorological data showed that the most striking difference between the majority of the nights when regular variations with time were observed and the remaining 35 per cent of the flights was the intensity of advection. This can be seen in Table 2, which gives a summary of the wind velocity at various altitudes for the night flights. The differences in the intensity of advection are especially evident above 5,000 ft, where the frictional influences of the surface have a negligible influence on the wind velocity. At 10,000 ft, for example, the average wind velocity for periods when the regular decrease of nucleus concentration with time occurred was 15 kt, with a maximum velocity of 25 kt, while in the cases of irregular variations with time the average velocity was 38 kt with a minimum of 24 kt. Most of the nights with irregular variations in nucleus content also showed a departure from the normal daily wind cycle at the surface. In many cases, the temperature gradient, fluctuations in the air-speed records and random motion of the aircraft showed that the increase in nucleus concentration with time was due to the fact that stability did not develop in the surface layers, so that vertical transport from the surface remained the dominant influence. This is illustrated in the results of 21, 22 Jan. 1954 given in Figs. 4 (a) to 4 (c), where a continuous increase in nucleus concentration with time is observed in the exchange layer during the night hours. The temperature profiles in Fig. 4 (c) show that stability is not developed near the surface and that there is intense temperature advection; the average wind velocity at 10,000 ft was 40 kt and the surface-wind velocity showed a secondary maximum at 2300 hr. During two series, unstable temperature gradients were produced by advection at higher levels, resulting in mixing and modification of the nucleus distribution, and in an extension of the height of the exchange layer.

TABLE 2. COMPARISON OF WIND VELOCITIES FOR NIGHTS WHEN REGULAR AND IRREGULAR VARIATIONS OF NUCLEUS CONCENTRATION WITH TIME OCCURRED

Altitude (ft)	Regular wind velocity (kt) limits of variation	average	Irregular wind velocity (kt) limits of variation	average
surface	calm–26	11	3–36	14
5,000	3–24	13	12–45	24
10,000	3–25	15	24–57	38
15,000	3–29	21	30–86	53

The analysis of individual series indicates that, during periods of intense advection, the frictional effects of high winds and temperature advection are largely responsible for the increase in nucleus concentration at night at a given level; the increased possibility of inhomogeneities in the air mass during such periods cannot however be excluded. It should be noted that the average winds for the non-systematic series given in Table 2 are approximately equal to the average value of the horizontal winds in winter. Non-systematic variations in nucleus concentration are, therefore, expected to occur most often during the winter months.

(*d*) *Above the exchange layer*

As shown in Figs. 2, 3, 4 and 6 and reported by Sagalyn and Faucher (1954), in fair weather the nucleus content of the atmosphere is low above the exchange layer. The time variations in charged nucleus concentration which occur at a given level in this region do not show a regular daily variation. The experimental results show that the vertical transport of nuclei through the top of the exchange layer is negligible. Above the exchange layer no correlation has been found between the magnitude of the daily variation in nucleus content and the intensity of advection.

During the course of the investigation, two methods were found important in introducing large numbers of nuclei to levels above the exchange layer:

(i) While the discussion has been limited to results in fair weather, series were also carried out when fronts approached or thunderstorms developed. In these cases, measurements were continued as long as possible and the results show that with the close approach of the disturbance the stable temperature gradient at the top of the exchange layer is broken up and large numbers of nuclei penetrate to higher levels.

(ii) If the exchange layer penetrates to height X on a given day, and if on the following day mixing penetrates to a lower height, Y, in the altitude interval $X - Y$, some of the characteristics of the distribution of the previous day will still be evident and can persist for several days. The nucleus concentration is quickly reduced by coagulation and there is no sharp concentration gradient in the vicinity of level X, so that the electrical properties in this region are not much affected. This influence of the previous history can also be seen in the morning flights before the exchange layer is fully developed, as shown for example, in the first and second flights of 20 August 1953 in Fig. 2.

5. Electrical conductivity results

The changes in electrical conductivity at a given level in the exchange layer are the inverse of the changes taking place in nucleus concentration; an increase in large ion concentration is accompanied by a decrease in conductivity and vice versa. Thus, at a given level the conductivity begins to decrease around sunrise, reaching a minimum at approximately 15 hr (Fig. 1 (b)). During periods of low advection, the conductivity increased until the following sunrise, whereas a more complicated time variation occurred during periods of intense advection. Examples of the time variations obtained on individual series are shown in the curves of Figs. 2 (b), 3 (b), 4 (b) and 6 (b).

Above the exchange layer the variations in electrical conductivity with time, as indicated in the figures mentioned, do not show a regular daily variation.

The minimum and maximum daily variations of electrical conductivity measured during the period of these experiments, in and above the exchange layer, are given in Table 1.

6. Discussion of results of Section 5

(a) *Effect of nuclei on electrical conductivity*

The observed relation between electrical conductivity and nucleus concentration can be understood upon examination of the theoretical equations which give the change of small ion concentration with time in terms of the processes by which these ions are normally produced and disappear in the atmosphere. For small positive ions :

$$\frac{dn_+}{dt} = q - \alpha\, n_+\, n_- - \eta_{10}\, n_+\, N_0 - \eta_{12}\, n_+\, N_- \quad . \quad . \quad (3)$$

where n_+ and n_- are the concentrations of positive and negative small ions, t is time, N_0 the concentration of uncharged nuclei, N_- the concentration of large negative ions (negatively charged nuclei) and q the rate of production of small ions. α is the volume-recombination coefficient for oppositely charged small ions, η_{10} and η_{12} are the attachment coefficients for collisions between small ions and uncharged nuclei, and charged nuclei of opposite sign, respectively. A similar expression can be written for small negative ions.

In regions of the atmosphere where (a) dn_+/dt is small compared to the other terms in Eq. (3), (b) $n_+ \simeq n_- = n$, and (c) $N_+ \simeq N_- = N$, the solution of Eq. (3) can be written :

$$n = \frac{-\beta N \pm (\beta^2 N^2 + 4\alpha q)^{\frac{1}{2}}}{2\alpha} \quad . \quad . \quad . \quad (4)$$

where $\beta = \eta_{12} + \eta_{10} N_0/N$. If the difference in mobility of positive and negative small ions usually found at the surface exists at higher levels in the atmosphere, then, as shown

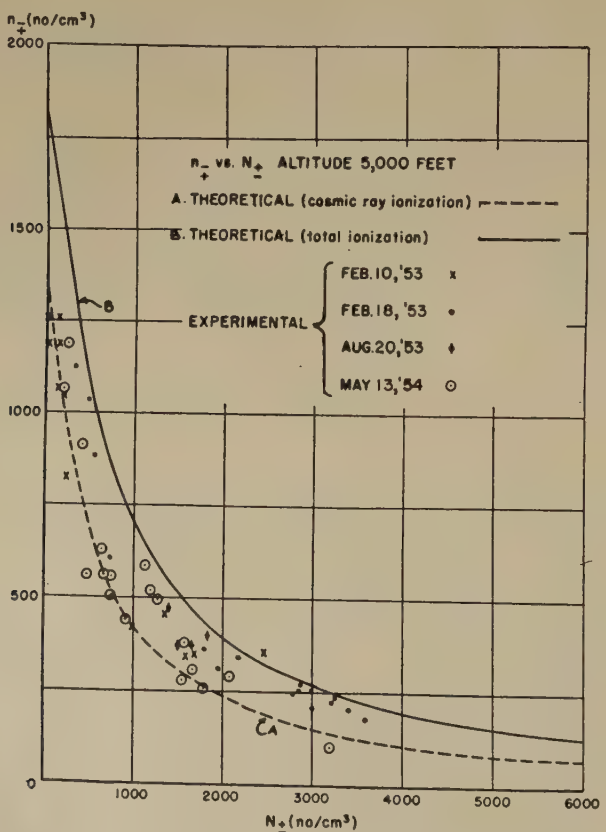

Figure 7. Small ion density ($n_\mp$) vs. charged nucleus concentration ($N_\pm$) at 5,000 ft.

by Bricard (1949) and others, in polluted regions $n_+/n_- = k_-/k_+$ (k_+ and k_- are the mobilities of positive and negative small ions, respectively). This small difference in positive and negative ion density is not important to the present discussion.

The evaluation of Eq. (4), for values of α and β representative of conditions at 5,000 ft, is given in Fig. 7. Curve A is the solution, using Neher's values, for the rate of production of ions due to cosmic radiation at a geomagnetic latitude of 50°, and Curve B is the result when Regener's (1934) experimental values for total ionization at geomagnetic latitude 49° are used. α was obtained from Thomson's theory (Loeb 1955) of volume recombination. The average mass of the ions and their mean free path must be given in order to obtain a solution to Thomson's equation. The average mass of ions in air was obtained by substituting Sayers's (1938) experimental values at STP in Thomson's expression for the recombination coefficient. The mean free path of small ions was assumed equal to one-third the mean free path of air molecules. Bricard's theory (1949) of the collision process between small ions and nuclei was applied to obtain β; for singly-charged nuclei with mean radii given in Table 3, this reduces to $\beta = 2\eta_{12}$,

where
$$\eta_{12} = \frac{4\pi Da}{\int_1^\infty \frac{1}{x^2} \exp - \frac{ek}{Da}\left\{\frac{1}{x} + \frac{1}{2x^2(x^2-1)}\right\} dx} \quad . \quad . \quad (5)$$

and $x = r/a$. This theory assumes that all small ions entering a sphere of radius r centred about a nucleus of radius a become attached to the nucleus. D is the small ion diffusion coefficient. The dependence of these processes on altitude is shown in the curves of Fig. 8, which represent the solution of Eq. (4) at 2,000, 5,000, 10,000 and 15,000 ft, assuming

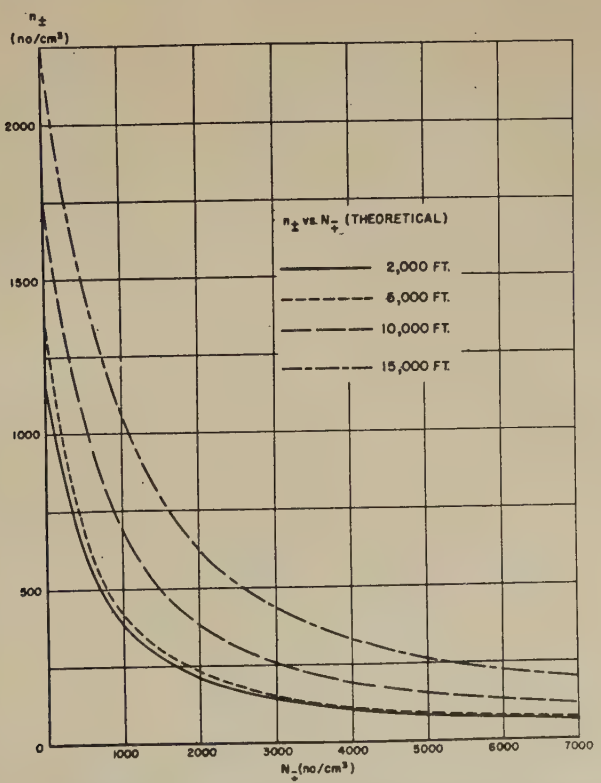

Figure 8. Small ion density ($n_\pm$) vs. charged nucleus concentration ($N_\mp$) at various altitudes.

cosmic radiation is the only source of ionization and using the computed values of α and η_{12} given in Table 3. The values of a given in Table 3, representing the average radii of charged nuclei, were computed from mobility measurements at these levels.

TABLE 3. VALUES OF MEASURED AND COMPUTED QUANTITIES USED IN THE SOLUTION OF EQ. 4

Altitude (ft)	q_0 ions/cm^3-sec at 20°C and 760 mm	k cm^2/sec-volt	a cm $\times 10^6$	η_{12} cm^3 sec^{-1} $\times 10^6$	α cm^3 sec^{-1} $\times 10^6$
2,000	2·9	1·3	2·5	3·2	2·1
5,000	4·5	1·5	2·4	4·3	2·1
5,000	8·1	1·5	2·4	4·3	2·1
10,000	8·8	1·7	2·2	4·2	2·2
15,000	17·3	2·0	2·1	4·1	2·2

The experimental results given in Fig. 7 represent simultaneous measurements of conductivity and large ion concentration on eight flights at 5,000 ft at a geomagnetic latitude of 53°. Flights were chosen where the mobility-distribution measurements showed the radius of maximum charged nucleus concentration to be approximately $2{\cdot}4 \times 10^{-6}$ cm. The small ion concentration was deduced from the conductivity data, using the fundamental equation for polar conductivity which gives :

$$n = \frac{\lambda}{e\,k_0}\left(\frac{T_0\,P}{T\,P_0}\right) \quad . \quad . \quad . \quad . \quad (6)$$

where λ is the polar electrical conductivity; e the charge per ion; k_0, T_0 and P_0 are the values of mobility, temperature and pressure at 760 mm Hg and 273° Absolute. $k_0 = 1{\cdot}3$ cm^2/sec-volt was used in these calculations. The results in Fig. 7 show good agreement with theory. For a given nucleus concentration, the small ion density is usually greater than that expected if cosmic radiation were the only source of ionization, and less than that expected assuming Regener's values for total ionization. This indicates a variation from day to day in the contribution of radioactive substances to the total ionization. Part of the variation could also be due to departures from equilibrium, since we have seen in the diurnal results that this is, in general, only an approximation in the exchange layer. The small ion mobility and volume-recombination coefficient at STP, assumed constant in these calculations, are also expected to vary slightly with changes in impurities, water vapour, etc.

In Fig. 8 it is seen that at a given level the relative decrease in small ion concentration, for a given increase in charged nucleus concentration, depends on the magnitude of the nucleus concentration. It should also be noted that an increase of a few hundred charged nuclei per cm^3 above the exchange layer will ordinarily produce a much greater change in conductivity than the same increase in the exchange layer.

These results are important in understanding the relative changes in large ion concentration and conductivity with time at all altitudes shown in Figs. 2, 3, 4 and 6.

(*b*) *Columnar resistance*

The influence of the daily changes in electrical conductivity on the atmospheric electric current system over continents can be summarized in terms of the columnar resistance of the atmosphere. The columnar resistance R of a vertical column 1 cm^2 in

cross section from the surface to 15,000 ft, equal to $\int_0^{15,000} 1/(\lambda_+ + \lambda_-)\, dh$, was computed from the conductivity measurements for all flights over the New Hampshire flight path. The experimental curves were extrapolated to obtain the conductivity at the surface. Simultaneous surface and free-air measurements have shown that this will not introduce a serious error and could not change the nature of the resulting columnar resistance variations to 15,000 ft. The average columnar resistance as a function of time was then computed and the results are given in Fig. 9. The general nature of the local daily variation is evident in this average curve; the columnar resistance starts to increase with time around sunrise, reaches a maximum at approximately 15 hr, then decreases with time to the following sunrise. It closely parallels the diurnal variation in nucleus content with time at a given level in the exchange layer during periods of low advection. The regular variation in conductivity with time from sunrise to 15 hr, and the more complicated nature of the variation through the late afternoon and night, which was found to depend on the intensity of advection, is also indicated in the scatter of the individual points of Fig. 9.

The daily variation in the total columnar resistance of the atmosphere varied from 13 to 60 per cent with respect to the average on individual series, with a mean value of 30 per cent. The columnar resistance above 15,000 ft was assumed to have no diurnal variation. The daily changes in the columnar resistance of the atmosphere in southern New Hampshire due to local changes in nucleus concentration are, therefore, of the same order of magnitude as the universal daily variation in the potential between the earth and upper conducting layers deduced from potential gradient and conductivity measurements over oceans (Gish 1951).

7. Time variations in the horizontal

(*a*) *Results*

Further support for the interpretation of the daily variation in the nucleus and conductivity distributions just presented is given by the nature of the changes with time in the horizontal fluctuations of nucleus concentration, electrical conductivity, humidity, and the instantaneous fluctuations about the mean in the air-speed records. Earlier measurements (Sagalyn and Faucher 1954) obtained during the daylight hours have shown that above the exchange layer in a given air mass in fair weather these parameters vary slowly in the horizontal. Over a fixed flight path approximately 25 mi in length, the magnitude of the variation at a given level usually amounted to less than 100 charged nuclei of one sign per cm^3 and to less than 10 per cent of the mean value of conductivity. In the exchange layer, in addition to the gradual variations observed at higher levels, large rapid fluctuations were observed, often amounting to more than 100 per cent of the

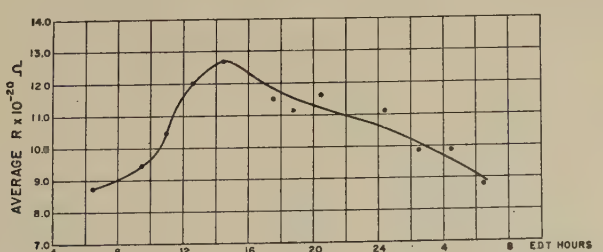

Figure 9. Average resistance (R) of an air column 1 cm^2 in cross section and 15,000 ft in height vs. time.

average value at a given level. These localized variations generally decreased with height in the lowest few thousand feet, then increased, reaching a maximum in the vicinity of the top of the exchange layer. Maximum horizontal fluctuations usually occur at, or slightly below, the altitude where a change to a more stable temperature gradient is observed. The region of large, rapid fluctuations is approximately 2,500 ft in depth. An example of the horizontal fluctuations of these parameters in the vicinity of the upper boundary of the exchange layer is shown in Fig. 12 of Sagalyn and Faucher (1954).

Extension of these measurements over 24-hr periods has shown that the magnitude of the fluctuations, and the altitude region where maximum fluctuations occur, vary with time. The height of the altitude region of maximum nucleus, conductivity, and water vapour variations, and the magnitude of these fluctuations, increase from around sunrise to approximately 15 hr. During periods of low advection, the magnitude of the fluctuations decreases from 15 hr to the following sunrise, while the height of the altitude region of maximum disturbance remains constant or is lowered. Also, in the early morning hours, the upper boundary of the exchange layer is no longer always distinguished by a change to a more stable temperature gradient. This is illustrated in Fig. 10, which gives the magnitude of the short-period horizontal variations in nucleus concentration, conductivity, humidity, and the instantaneous fluctuations about the mean in the airspeed

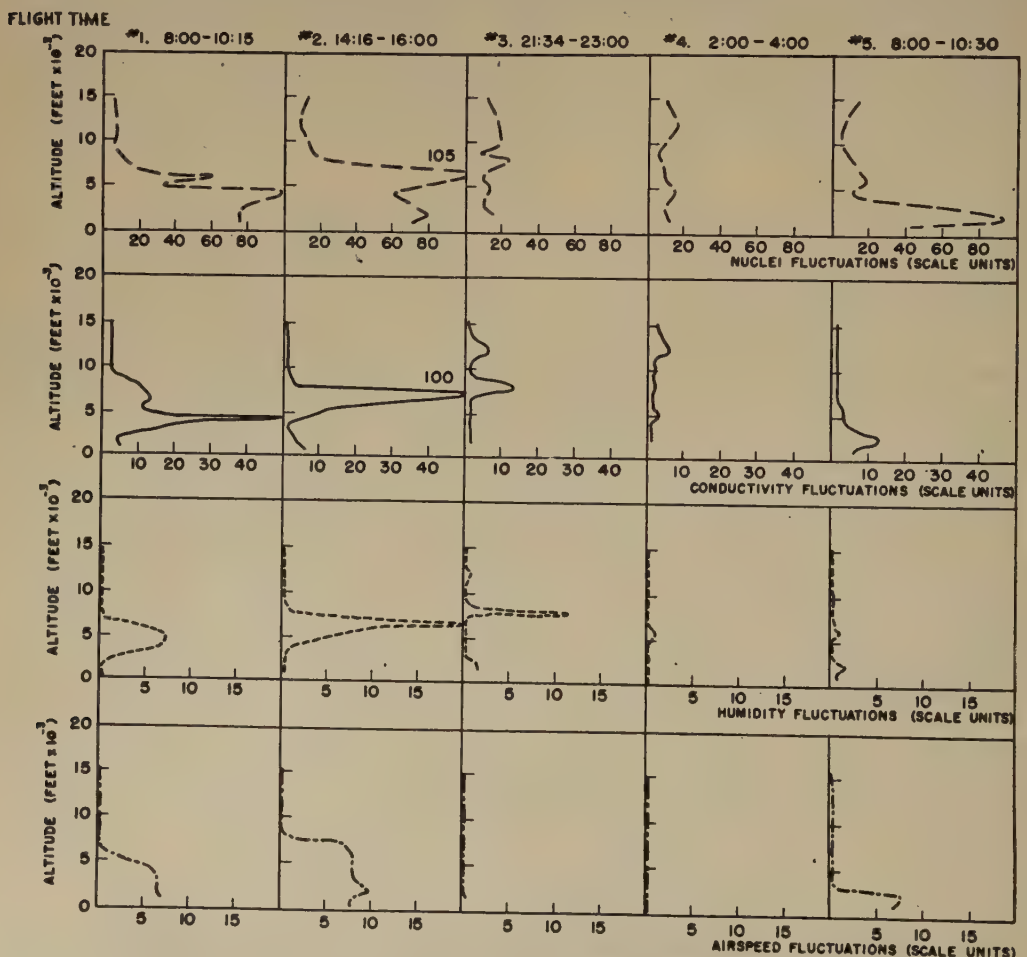

Figure 10. Horizontal fluctuations of nuclei, conductivity, humidity and air speed, as a function of time, 13-14 May 1954.

records, for 13, 14 May 1954 – a period of low advection – as a function of altitude and time over a 24-hr interval. The values given in Fig. 10 for the nucleus, conductivity, and humidity fluctuations represent the maximum fluctuations over 1·25 mi of flight path. The instantaneous air-speed fluctuations are given as a qualitative indication of the existence of turbulence. Since we only wish to point out the nature of the horizontal variations with time, the record deflections have not been converted to absolute units. During periods of intense advection the horizontal fluctuations were not found to vary regularly with time.

(*b*) *Discussion*

(i) *Variation in height with time*

The increase in the height of the exchange layer (the region of maximum horizontal fluctuations) from around sunrise to early afternoon follows the well-known daily increase in the intensity of atmospheric turbulence (Sutton 1951). As illustrated in the results of 13, 14 May 1954, shown in Fig. 10, individual series show that the variations in the height of the exchange layer during this period follow closely the variations in the height to which turbulent mixing extends, as obtained from the air-speed records.

When the intensity of turbulence decreases, the atmospheric pressure system in the area becomes important in determining any further changes in the height of the layer. As pointed out in Section 4, these experiments show that the exchange layer is often lowered by subsidence from late afternoon to the following sunrise.

(ii) *Variation in nucleus fluctuations with time*

From sunrise to approximately 15 hr, the horizontal fluctuations of nucleus count are large at all altitudes in the exchange layer. The horizontal fluctuations and their variations with time are greatest, however, in the vicinity of the upper boundary of the exchange layer. This can be considered as a region of very incomplete mixing, the boundary between two areas with very different nucleus properties. The increase in the intensity of turbulence during this period causes an increase in the vertical transport of nuclei which results in greater concentration gradients in the boundary region. When turbulence decreases in the afternoon, the inhomogeneities at all altitudes in the exchange layer also decrease, probably by diffusion.

(iii) *Variation in conductivity fluctuations with time*

The horizontal fluctuations in electrical conductivity and their variations with time, as indicated in Fig. 10, are normally much greater in the vicinity of the upper boundary of the exchange layer than at lower levels. This can be easily understood upon examination of the solutions of the ion balance relation shown in Figs. 7 and 8. Here we see that the magnitude of the change in small ion content, and therefore electrical conductivity, for a given change in nucleus concentration, depends upon the absolute value of the nucleus content. In the lower levels of the exchange layer the nucleus concentration is ordinarily much greater than in the vicinity of the upper boundary of the exchange layer. Using reasonable atmospheric values, it can be seen, for example, that an increase in charged nuclei from 500 to 1,000 per cm^3 results in a loss of 280 small ions of one sign per cm^3, while an increase of charged nuclei from 2,000 to 2,500 results in a loss of only 50 per cm^3. The magnitude of the change in the small ion concentration for an increase of 500 charged nuclei per cm^3 differs by over a factor of 5 in the two cases. Similar analysis shows that the variations in the conductivity fluctuations with time are in good agreement with those expected theoretically for the observed fluctuations in nucleus concentration.

During periods of intense advection, these experiments show that during the late afternoon and night hours, mechanical turbulence and temperature instability are often great enough to maintain essentially daytime conditions.

8. Relation to surface measurements

The daily variation of nucleus concentration at constant altitude described in this paper differs in some respects from the most commonly reported daily variation measured at low-elevation surface stations in fair weather. The latter shows a maximum in the morning, a minimum early in the afternoon, an evening maximum followed by a steady decrease in nucleus concentration until sunrise.

The present investigation shows that the decrease in nucleus concentration in the middle of the day has been correctly attributed to the vertical transport of nuclei by turbulence from regions where the supply of nuclei is limited. The results also indicate that at some altitude between the surface and approximately 700 ft there is a change from a decrease of nucleus concentration with time in the middle of the day to a variation of the type described in Fig. 1. The decrease with time from late evening to sunrise has been attributed to the settling of nuclei under gravity (Bradbury and Meuron 1938), decrease in the production of nuclei (Landsberg 1938) and to an increase in the austausch coefficient during the night hours (Franke 1949). While the last process may at times contribute to the decrease, comparison of available surface data with the theoretical coagulation curves indicates that coagulation is often the most important process causing the decrease in concentration during the night hours.

The reported daily variation of nucleus concentration at mountain stations in fair weather is essentially the same as that reported here for altitudes above 700 ft in the exchange layer and the same explanation should be applicable (Landsberg 1938).

The daily variation of nucleus concentration at surface stations is thus the result of the action of several competing processes. The principal ones in fair weather are : the nature of the source of nuclei, vertical transport of nuclei by turbulence and convection, nucleus coagulation. Each of these is the controlling influence at different times of day. It should also be pointed out that the loss of nuclei by coagulation must be included in any explanation of variations of small ion density, electrical conductivity or electric field at surface stations during the night hours.

9. Conclusions

An investigation of the time variations of charged nuclei and electrical conductivity in the altitude range 700 to 15,000 ft above the surface has shown :

1. In the exchange layer at a given level the concentration of charged nuclei increases with time from around sunrise to approximately 15 hr. During periods of low advection the concentration decreases from 15 hr to the following sunrise. Analysis shows that the increase in nucleus concentration to 15 hr is due to the transport of nuclei from the lowest levels by turbulent and convective mixing and the following decrease is due primarily to nucleus coagulation and subsidence. During periods of intense advection, the nucleus concentration does not vary in a regular manner from 15 hr to the following sunrise. The frictional influence of high winds prevents the cutting off of nuclei from the lowest levels and in some cases temperature advection produces unstable temperature gradients at higher levels in the atmosphere. Above the exchange layer the nucleus variation with time is greatly reduced and is not found to be regular.

2. The variations with time in the electrical conductivity are the inverse of the changes in charged nucleus concentration. Application of the ion balance relation and the theories of Thomson and Bricard for collisions between small ions and between small ions and nuclei shows that the conductivity variations result primarily from loss of small ions by collisions with nuclei. The columnar resistance of the atmosphere computed from the conductivity data is found to have a simple local daily variation during periods of low advection with a maximum at approximately 15 hr. The amplitude of the daily variation in the columnar resistance of the atmosphere is of the same magnitude as the universal daily variation in the potential between the earth and upper conducting layers in fair weather, and therefore is an important factor contributing to variations in the atmospheric electric current system over continents.

3. In the exchange layer, over a fixed flight path the horizontal inhomogeneities in number of nuclei, conductivity, and water vapour content have a regular daily variation during the periods of low advection.

4. The height of the exchange layer increases with time from sunrise to 15 hr during periods of low advection; then remains constant or is gradually reduced, depending on the pressure system in the vicinity of the flight path, until the following sunrise.

5. The upper boundary of the exchange layer also corresponds to the upper limit of the scattered fair-weather cumulus cloud layer. An increase in nucleus concentration and large horizontal variations in nuclei, conductivity and water vapour are found at scattered cloud levels. In such regions, an increase in nuclei corresponds to an increase in water vapour.

6. The magnitude of the nucleus concentration is not found to be a conservative air-mass property.

Acknowledgments

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Radar observations of showers suggesting a coalescence mechanism

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(Manuscript received 12 December 1955, in revised form 22 March 1956)

Summary

Radar evidence suggesting the production of showers by the coalescence process is presented. On 13 days during the summer of 1955, 48 separate echoes were observed to *originate* below the — 2°C level and 26 of these remained there during the whole life of the shower. These echoes, which were often associated with mixed formations of cumulus and stratocumulus cloud, sometimes extended above the 0°C level at their maximum state of development, but surpassed the — 8°C level on only one occasion. Some of the limitations of using radar alone in this type of study are discussed.

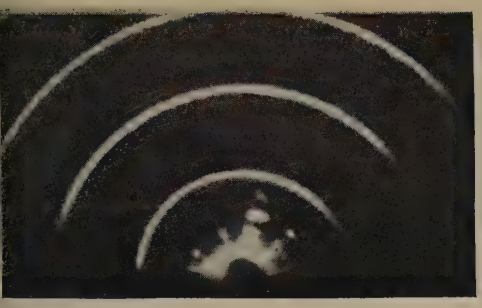
1. Introduction

There are very few well-documented cases of showers falling from non-freezing clouds in this country; the only published accounts are those by Jones (1951) and by Browne, Day and Ludlam (1955). There are, however, good reasons for thinking that, in warm weather, conditions favourable for the release of showers by the coalescence process may often exist. Calculations by Ludlam (1951) indicate that showers may be initiated in clouds little more than 1½ km deep, having base temperatures about 10°C and mean updraughts of 1 to 3 m/sec, by large hygroscopic nuclei entering the cloud base and growing by condensation to radii exceeding 30 μ when lifted a further few hundred metres. Since few parts of the British Isles are more than 100 mi from the sea, there should generally be a sufficiency of such nuclei produced by sea spray. Indeed, cloud-droplet samples taken over southern England show that cumulus, stratocumulus, and, in particular, mixed formations of both cloud types, commonly contain appreciable numbers of droplets with radii exceeding 50 μ (Murgatroyd 1955). There is also evidence (Ludlam and Saunders 1956) that such droplets may arise independently of the presence of giant hygroscopic nuclei.

It was therefore felt that a systematic search might reveal showers to be *initiated* by the coalescence of water drops much more frequently than is generally supposed. There may, of course, be occasions on which the precipitation is initiated by the coalescence process, after which the cloud continues to grow and later allows the ice-crystal mechanism to intervene. In the absence of aircraft facilities, it was thought that a pointer to the primary rain-forming mechanism might be obtained by locating the first detectable radar echo relative to the 0°C level; some justification for this procedure and a discussion of its limitations are at the end of the paper. The results of such an investigation, made during the summer of 1955, are described.

2. Equipment and observational procedure

The equipment used during the investigation was a 3-cm radar installed on the roof of the Meteorology Department, Imperial College, S. Kensington and the 10-cm equipment of the Meteorological Office Radar Station, East Hill, near Dunstable. The former is a modified aircraft radar, the aerial of which sweeps in a vertical plane through the zenith to provide a vertical cross-section of the atmosphere on a chosen bearing as an RHI display; alternatively, the aerial can be locked in a fixed position to provide an A-scope presentation. The set has a peak power of 10 kw, a pulse length of 1 μ sec, and a conical beam of width



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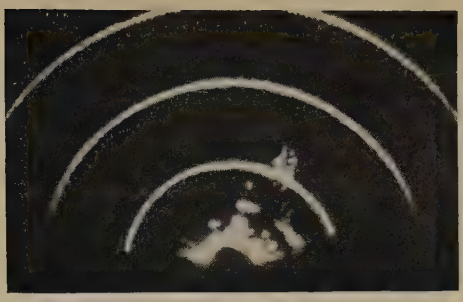

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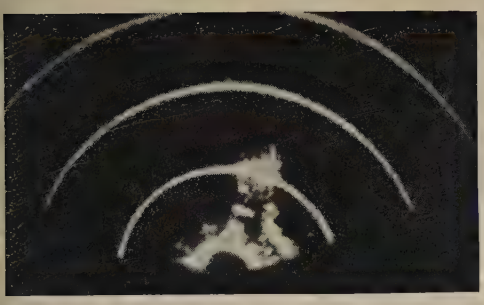

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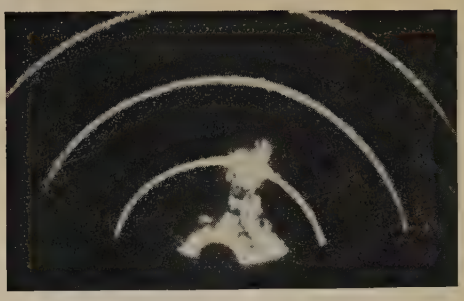

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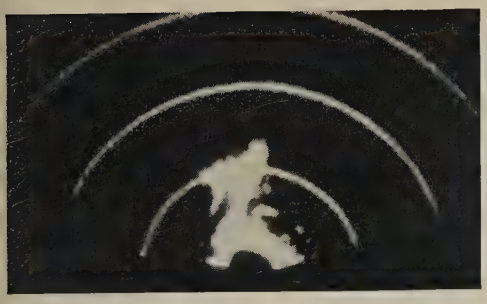

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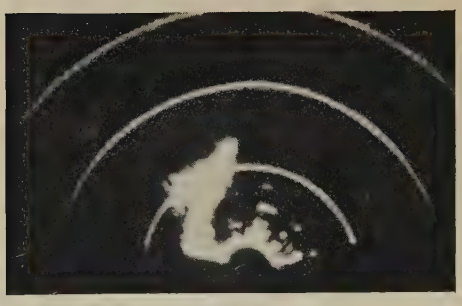

(f)

Figure 1. A sequence of photographs showing the development of coalescence showers on the Imperial College RHI radar display, 11 Aug. 1955. The range markers are 2 mi apart.

(a) 1010.00 hr Echo not yet apparent ; (b) 1013.30 hr Initial echo top at 12,500 ft, — 2°C ; (c) 1014.30 hr Echo top at 13,000 ft ; (d) 1015.00 hr Top at 13,000 ft but echo spreading downwards ; (e) 1016.15 hr Top still at 13,000 ft but echo spreading laterally and downwards ; (f) 1019.00 hr Echo at maximum state of development. Top at 14,500 ft, — 5·5°C.

4° to half power. For mechanical reasons the aerial sweep is restricted to 75° on either side of the vertical – this means that its useful working range is less than 10 mi, at which distances only the tops of clouds extending above 15,000 ft can be detected. The cathode-ray tube displays are photographed at suitable time-intervals on 35 mm film on which measurements are made under a microfilm reader. A repeater RHI display is watched continuously by the observer. While this equipment is very suitable for studying the structure and development of nearby storms, its limited range allows us to explore only a comparatively small area. Therefore, during June and July 1955, observations were also made at East Hill with the 10-cm sets having both PPI and RHI facilities, as described by Jones (1950).

The procedure adopted was to select, visually, a suitable bulging cumulus, direct the RHI aerial on to it and wait for the appearance of an echo. On a few occasions it was also possible to make theodolite observations on growing cumulus towers and measure their rates of ascent. When working at longer ranges or in poor viewing conditions, the initial search was made with the PPI radar and as soon as the echo appeared on it, the RHI was immediately directed along that particular bearing. In some cases, therefore, it was not possible to make measurements on the echo at the instant of its first appearance, but, in general, the delay was less than 20 sec.

In these observations the sets were operated at maximum gain and the heights of the echo base and top could be determined from photographs taken at one-minute intervals. A typical sequence is shown in Fig. 1.

3. Analysis of observational data

On 13 days during the period May-September 1955, first detectable echoes from shower clouds were found to lie either below or within 2°C of the 0°C isotherm. These are listed in Table 1, which gives the height and temperature of both the base and top of the initial echo, similar data for the echo at the state of maximum vertical development, and the height and temperature of the visual cloud base. Additional information includes the type of cloud formation, the position of stable layers which, together with the temperature data, were given by the nearest radiosonde ascent (Crawley or Hemsby at mid-day).

It will be seen that these shower clouds had base temperatures always higher than 5°C and usually above 10°C. Of the 48 separate echoes which were observed to originate below the − 2°C level, 26 remained there during the whole life of the shower, but 14 were subsequently observed to extend into the sub-freezing regions of the cloud. However, in only one case did the top of the observed echo become colder than − 8°C, and in no case was a melting band or upper band (precipitation streak) detected. On only four of the days (29 June, 13 July, 11 August, 9 September), involving 4 of the 48 echoes listed in Table 1, were echoes extending to greater heights than those given in column 10, observed to appear before the echoes under discussion, and three of these four echoes were more than 50 mi away.

The vertical depths of the first detectable echoes ranged between 200 m and 2,000 m (these depths often being a consequence of the beam width at the range in question) and usually these spread vertically in both directions, the bases often reaching the ground. The rates of ascent of the echo tops varied between 1 and 5 m/sec. The possibility that the maximum vertical development of the echoes may have been underestimated because of attenuation of the radar beam by intervening precipitation may be disregarded, particularly with the 10-cm equipment, because the precipitation was usually of low intensity and the isolated showers were generally observed at a distance and not overhead. The heights of the tops of echoes observed by the Imperial College radar may have

TABLE 1. DETAILS OF RADAR ECHOES FROM COALESCENCE SHOWERS

Date	Time	Ht of 0°C level (km)	Cloud base Ht (km)	Cloud base Temp (°C)		Base of initial echo Ht (km)	Base of initial echo Temp (°C)	Top of initial echo Ht (km)	Top of initial echo Temp (°C)	Maximum height of echo Ht (km)	Maximum height of echo Temp (°C)	Position of stable layers	Cloud type	Remarks
27.5.54*	2130	3·1	1.1	14·5		1·9	9	2·1	8	4·5	− 8	None below 5 km	7/8 Sc. cast.	Few spots of rain.
4.5.55*	1415	1·7	0·8	6		1·2	3	2·0	− 1	3·2	− 8	Isothermal 3-3·6 km	6/8 Mixed Cu and Sc.	Rainfall 7 mm/hr.
5.5.55*	1518	2·1	1·0	7		2·0	0·5	2·4	− 1·5	2·8	− 3	Isothermal 2-2·9 km	6/8 Cu and Sc. cloud streets.	Rainfall 3 mm/hr Cu tops showing strong wind shear.
9.5.55*	1702	2·4	0·8	8·5		Not observed				2·2	1·5	Isothermal 2·7-2·9 km	Cu spreading to Sc. No medium cloud	2 similar echoes observed. Trace of rain.
27.6.55	1049 to 1708	3·0	0·9	9	5 echoes ranging from	0·9 to 1·5	9 to 5	2·1 to 3·0	3 to 0	2·1 to 3·0	3 to 0	Inversion 2·9-3·1 km	Cu spreading under wind shear. Sc forming in between. Later 8/8 Sc.	3 other echoes with tops below 2·7 km (1°C) and producing rain at ground.
29.6.55*	1414	3·3	0·6	13		1·0	10	3·0	0·5	4·5	− 8	Stable 2·9-3·1 km	6/8 Cu and Cb.	Heavy shower at ground. Only echo observed previously reached 4·5 km, 60 mi east at 0930.
7.7.55	1430	4·0	1·4	13		Not observed – 2 echoes extending to ground				2·1 1·8	10 11·5	Isothermal 2·4-2·6 km	Large Cu penetrating Sc. deck.	
	1522					Not observed – echo extending to ground				1.5	13			
12.7.55	1655	3·7	1·0	15		2·1	10	4·2	− 2	4·5	− 5·5	None.	3/8 small Cu. Echoes produced by isolated cluster of large Cu.	3 other echoes also observed. See Table 2 for measurements on visual cloud.
	1717					Not observed				3·6	+ 0·5			
13.7.55	1325	4·4	1·0	16		Not observed				4·8	− 1·5	None.	5/8 Cu cong.	Other clouds in neighbourhood developed into thunderstorms.
27.7.55	1522 to 1557	3·9	1·0	15	3 echoes	1·5	12	3·6	3	3·6 to 4·2	3 to − 1	Isothermal 3·9-4·4 km	Large Cu cong.	3 other echoes developing at same time did not surpass − 8°C level.
28·7·55	1700 to 1717	3·7	0·9	15	4 echoes ranging from	0·9 to 1·5	15 to 11·5	3·0 to 3·6	4·5 to 0·5	Only one observed 4·5	− 3·5	None	Cu protruding through medium cloud deck.	
2.8.55	1307 to 1746	3·8	0·9	15	16 echoes ranging from	0·9 to 2·4	15 to 8·5	2·1 to 3·9	10 to − 0·5	2·1 to 4·8	10 to − 5·5	Isothermal 2·1-2·7 km	Cu cong. spreading out to form Ac layer.	Only echo observed previously reached 4·5 km (− 5°C).
11.8.55*	1013	3·2	0·6	14·5		2·4	5	3·6	− 2	4·3	− 5·5 (?)	Isothermal 3-3·3 km	8/8 mixed Cu and Sc.	Thunderstorm echoes passed at 10 mi from observer about this time.
9.9.55*	1411	2·9	0·9	10		1·0	9	3	− 0·5	5·1	− 14·0	None.	Cu cong. and scattered Cb.	Thunderstorms developed in vicinity later. Echo extending to 5·7 km observed by East Hill, 90 mi NW of London.

* Observations made with Imperial College radar; other observations made with East Hill equipment.

been underestimated because of range attenuation, but this would not lead to serious errors in locating the initial echo in those cases when the vertical extent of the latter was small. With the East Hill radar, working at much greater ranges, such errors were unimportant.

In a number of cases the level of maximum echo development appeared to lie a few hundred metres below a stable layer, but there were occasions on which the echo top, having paused at the stable layer, subsequently pushed through to considerably greater heights. These latter cases were usually associated with the formation of either stratocumulus or altocumulus decks by the spreading out of cumulus. The occurrence of non-supercooled shower clouds with canopies of stratiform cloud has been noted previously by Mordy and Eber (1954) and by Browne, Day and Ludlam (1955). This may, perhaps, be explained by the turrets of the growing cumuli, and the incipient raindrops which they may contain, being less likely to evaporate after the formation of the protective cloud layer.

On 12 July it was possible to make visual observations and measurements with a theodolite on the tops of growing cumulus towers, the height of cloud base being reported by an aircraft. The maximum rates of ascent of these turrets, and the depth of the visual cloud at the times of initial and maximum-echo development are given in Table 2.

TABLE 2. VISUAL AND RADAR DATA FOR SHOWER CLOUDS ON 12 JULY 1955

Time	Initial Echo			Maximum Echo			Maximum rate ascent of Cu. towers (m/sec)
	Base km	Top km	Cloud depth km	Base km	Top km	Cloud depth km	
16·32	2·4	4·5	4·9	2·1	4·5	4·9	6·5
16·38	1·0	4·7	5·1	–	4·7	5·2	4·2
16·43	1·2	4·4	4·7	0	4·7	5·2	2·0
16·55	2·1	4·2	5·2	1·2	4·5	5·7	4·0
17·17	–	–	–	0	3·6	4·8	3·6

4. Discussion

Although the radar echoes from the shower clouds listed in Table 1 suggested that the rain was initiated by coalescence in the non-supercooled part of the cloud, it should be pointed out that the sensitivities of the radars were such that they could detect drops only when they had attained precipitation size* and that they could not follow the very early stages of the process. One might therefore argue that the appearance of an initial echo below the 0°C isotherm does not provide conclusive evidence of the rain being formed by the coalescence process, but that the drops may have originated at higher levels as ice particles which, because of their lower reflectivity and range attenuation effects, fell undetected until they reached the melting level. The matter could, of course, be settled by showing that the *cloud tops* were also below the 0°C isotherm but unfortunately, without aircraft, it was not often possible to determine the heights of the cloud summits. However we believe that, for the clouds listed in Table 1, precipitation was initiated by coalescence and not by ice crystals for the following reasons :

(1) It is unlikely that the drops producing the radar echo could have resulted from the melting of single ice crystals since the latter, having fall speeds usually less than

* The Imperial College radar has detected drizzle consisting of drops about 200 μ radius falling at the rate of 0·2 mm/hr at a range of 1 km.

50 cm/sec, would be unable to fall towards the melting level through the updraughts to be expected in an active shower cloud. Although downdraughts might appear near the top of a growing cumulus as the result of evaporation of a cloud tower, and surviving ice crystals might be carried down during the interval preceding the ascent of a new bubble, calculation suggests that during this time, these crystals would not increase by more than about 50 μ in radius by sublimation and would be most unlikely to become wet. They would therefore have to melt before being detected and should then produce a melting band. None was observed.

(2) If snowflakes and small graupel (diameter < 2 mm) having fall speeds of about 1 m/sec were formed in the upper parts of the cloud, calculations indicate that, for a precipitation rate of a few mm/hr, they should have been detected by the radars at the ranges in question. Melting of the snowflakes should have produced a melting band.

(3) With the top of an initial echo appearing near the 0°C isotherm and the average temperature of its base being about 10°C, it seems reasonable to assume that the reflecting particles were drops, all formed by the same process. There was nothing in the appearance and development of the echoes to suggest the arrival of detectable particles just at the 0°C level and the subsequent spread of the echo towards lower levels.

(4) On 9 of the 13 days, stable layers were located usually below the — 5°C level; these might be expected to check vertical growth of the clouds and were, no doubt, responsible for the spreading out of the cloud tops which was observed on some occasions. It seems, therefore, unlikely that the cloud tops extended much above the — 10°C level, support coming from the fact that, apart from one occasion, the radar echoes never surpassed the — 8°C level.

Thus, although the observations on the location of the initial radar echoes do not of themselves provide unmistakable proof that the precipitation was started by the coalescence process, when taken together with the other relevant facts, they point strongly to this conclusion. However, the authors wish to emphasize the limitations of using radar alone to study the initial stages of precipitation development and the nature of the responsible physical processes. The stage in the development of the precipitation process at which a radar echo will first appear will obviously depend upon the range and reflectivity of the cloud and upon the performance characteristics of the particular radar. It is clear that, in order to obtain really conclusive information of the kind discussed in this paper, the radar observations should be closely coordinated with detailed examination and measurement of the visual characteristics of the cloud. Indeed, under favourable viewing conditions, the eye may be a more sensitive detector of the onset of precipitation than a centimetric radar. Such a combined study is now being planned.

It is of interest to note from Table 1 that the smallest vertical depth of an echo at its maximum state of development was 1·2 km (4,000 ft). On this occasion the depth of the visual cloud was not measured, but one may safely assume that it was at least 5,000 ft – the minimum depth necessary, according to Ludlam (1951), for the initiation of a coalescence shower in clouds with base temperatures 10° to 15°C, mean updraughts of 1 to 3 m/sec and containing droplets of 20-30 μ radii near cloud base. All the clouds listed in Table 2 were at least 2 km deeper than the minimum values required by Ludlam's simple theoretical model.

Acknowledgments

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Fog precipitation on Table Mountain

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Summary

Fog precipitation on Table Mountain has been measured over a period of one year by means of two raingauges, one of which was equipped with a fog-catcher. The evaluation of the records is complicated by the uncertainty of the fog precipitation during rain and drizzle. It is shown that satisfactory results can be obtained if at least two sets of instruments are installed on the mountain, one of them in the middle of the plateau and the other some 200 m lower.

The fog precipitation for the year on the plateau was found to be 3,294 mm and the rainfall to be 1,940 mm. The mean intensity of rainfall was 1·84 mm/hr and that of fog precipitation 3·75 mm/hr. Although the latter appears to be rather high, it is much lower than the value obtained theoretically. The discrepancy is accounted for by several errors in the measurements, which are discussed, and by fog precipitation supposed to occur on the slopes, which causes the air to reach the plateau with a smaller liquid-water content than calculation indicates.

1. Introduction

The Table Cloth is one of the orographic clouds which are formed in mountain regions all over the world. If, on a fine summer day, air from the SE is forced up Table Mountain under such conditions that condensation can occur on the slopes, the Table Cloth appears. The further ascent is wet-adiabatic, and the water droplets in the air will increase in number, and possibly in size, until the plateau is reached. A certain portion of the cloud droplets which move with the ambient air over the mountain may precipitate on the ground, mainly owing to eddies, but the droplets will tend to be deposited on all obstacles, such as rocks and plants, in the path of the moving air. Vegetation is much better suited than are rocks to catch cloud droplets. Because grass, leaves and twigs move in the wind, and so cause changing turbulent eddies, more water droplets will be deposited by precipitation and contact with the huge surface of plants than on compact obstacles.

Marloth (1904, 1907) noticed the considerable amount of water which was deposited, especially on the vegetation, when the mountain was covered with the Table Cloth in the absence of rain. He arranged a bundle of grass between wires upon the funnel of a raingauge and gathered with this instrument much water from the cloud, whilst an ordinary raingauge collected only little. Since then it has become the practice to measure fog precipitation by means of two raingauges with a fog-catcher attached to one of them. Dieckmann (1931) used a gauze cylinder, with a diameter two-thirds of that of the raingauge funnel, projecting 35 cm above the rim of the funnel. He found, as did Marloth, that when fog was present in the absence of rain the gauge with the gauze cylinder collected an appreciable quantity of water, while the standard raingauge showed little.

Grunow (1952) used a gauze cylinder of which the height was twice the diameter, and the vertical cross-sectional area was equal to the catching area of the raingauge to which it was attached. When rain falls at an angle (α) greater than 8° from the vertical the raingauge with the fog-catcher has an increased catching area (see Fig. 1). However, Grunow found that in the absence of fog the raingauge with the fog-catcher collected about the same total amount of rainfall as the standard raingauge over a period of one year, though single comparison shows appreciable differences. This discrepancy is probably due to the nature of the rainfall. For example, when large raindrops fall in strong winds

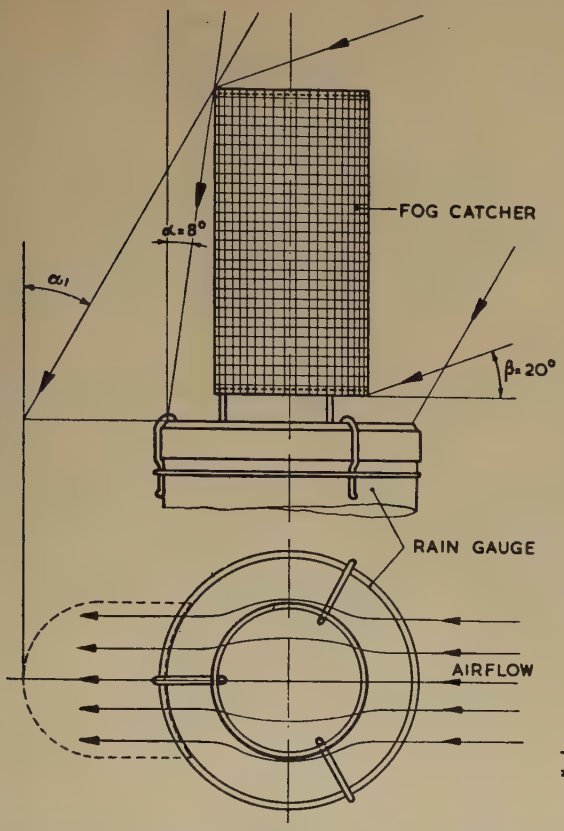

Figure 1. Fog-catcher on raingauge. The area surrounded by the dashed line shows the increment of the catching area due to the fog-catcher for rain falling at angle (α_1).

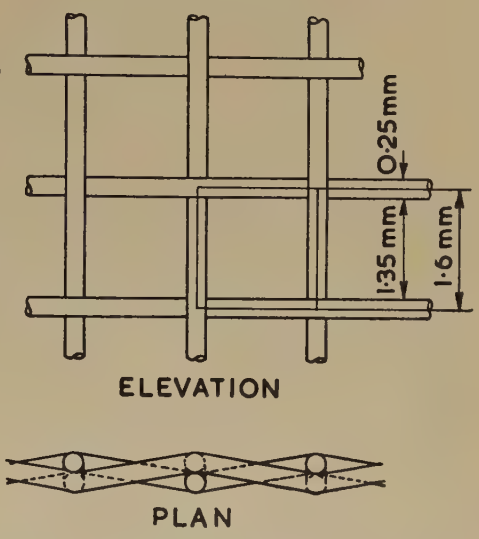

Figure 2. Portion of the gauze on an enlarged scale (Elevation and plan).

some of the drops will be deflected and blown over the funnel, and during light rainfall some of the water which adheres to the gauze will evaporate. On the other hand, when raindrops fall in moderate to fairly strong winds it is most likely that the raingauge with the fog-catcher actually collects more water than the standard raingauge, even though evaporation takes place and a number of the raindrops may be deflected by the gauze. However, when the instruments are within the cloud, Grunow subtracts the precipitation measured with the standard raingauge from that obtained by the raingauge with the fog-catcher without taking into account whether rain falls or not. As he considers this difference to be the amount of fog-precipitation, it appears that continuous observation is necessary to ensure whether fog is present or not.

2. The efficiency of the fog-catcher

Fig. 2 represents a portion of the gauze used in the fog-catcher (see Fig. 1). From the dimensions of the mesh it is found that 29 per cent of the surface of the cylinder is covered with the wires of the gauze and 71 per cent remains for free passage of air. When air containing water droplets flows with a speed of about 10 m/sec against the fog-catcher, eddies will form at the gauze, preventing a smooth airflow through the meshes and increasing the efficiency of the catch. This effect, although important, is disregarded.

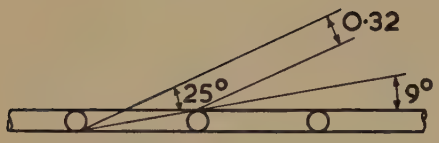

Figure 3. Simplified pattern of the gauze (plan).

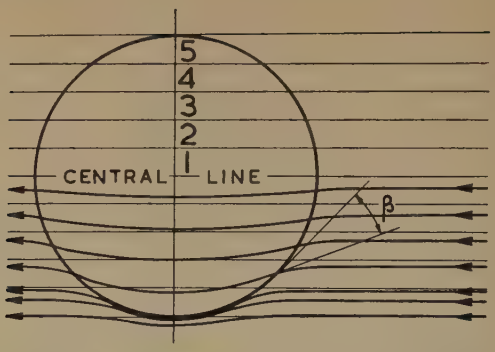

Figure 4. Representation of the laminar airflow through and along the sides of the gauze cylinder.

For the following calculation a simplified pattern of the gauze is considered as shown in Fig. 3, and a laminar airflow is assumed to pass through the meshes and along the sides of the cylinder. The airflow is thus less deflected than by a solid cylinder. On the other hand, it will be shown that only a small portion of the water droplets in the air which strikes the cylinder can pass through it and, therefore, a deflection of air around the cylinder still occurs. In Fig. 4, which represents the airflow, the catching area of the cylinder is subdivided into 10 equal sections (5 to each side of the central line). The angles (β) which are formed between the stream lines and the gauze are by no means under-estimated. When the airstream is normal to the gauze, 71 per cent of the cloud droplets can pass through the meshes as mentioned above; but when the air strikes the cylinder at an angle, β, less than 90°, the percentage which is able to pass through the gauze will be less. Cloud droplets cannot pass through the gauze if β is smaller than 9° (see Fig. 3). For different angles β, the distance (1·35 mm) between the two wires of a mesh has, therefore, to be multiplied by a factor which has been determined graphically. The following Table gives β for the 10 sections of Figure 4 and the corresponding factors for each section.

Section	β (degrees)	Factor
1	82	0·99
2	63	0·87
3	44	0·64
4	25	0·32
5	6	0·00
		2·82

The average factor is 0·56, which gives, when multiplied by 71 per cent, the result that 40 per cent of the cloud droplets which strike the cylinder can pass through the gauze in front. Applying the same calculation to the lee side of the cylinder, the result is obtained that 16 per cent of the cloud droplets in the air which is blown against the fog-catcher can pass through it. However, soon after the inception of the fog, water-drops appear on the gauze mainly on the windward side. These drops increase in size by coalescence with newly arriving droplets and partly close the meshes. The waterdrops which adhere to the gauze grow until they reach a size large enough to enable them to flow down the gauze and drop into the funnel of the raingauge. It has been observed that in moving cloud almost all the meshes on the windward side of the gauze cylinder were covered with waterdrops, while on the lee side only a few drops were present. This

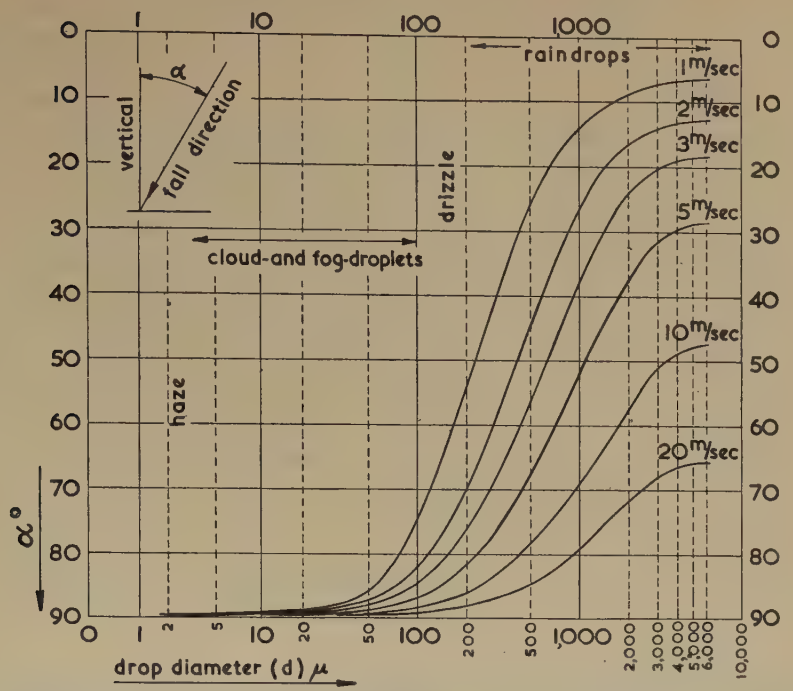

Figure 5. The graph represents the angle (α) at which cloud and rain droplets fall in a horizontal, laminar airstream of different speed as a function of the equivalent drop diameter d in μ. The scale of the abscissa is: $\log (1 + d)$.

implies that a much smaller percentage of cloud droplets than the above calculation indicates is capable of entering the gauze cylinder. Probably less than 10 per cent of the cloud droplets which are blown against the fog-catcher can pass through it.

If gauze with a larger mesh is used, more cloud droplets can pass through the fog-catcher. On the other hand, fine mesh, e.g., petrol gauze, is not suitable for this purpose as the fog-catcher will then nearly act as a solid cylinder. In this case, the airflow is more deflected, and some of the waterdrops which have formed on the gauze will be blown to the sides and off the cylinder without falling into the funnel of the rain-gauge.

For the two reasons, namely that the air which is blown towards the fog-catcher is partly deflected around the cylinder without touching the gauze (see Figs. 1 and 4), and that a certain percentage of the cloud droplets can pass through the meshes, the effective catching area of the gauze cylinder is smaller than that of the raingauge. Therefore, the fog measurements should show a deficiency estimated to be about 25 per cent for a wind speed of 4 m/sec, increasing for higher speeds. It is obvious that measurements with the apparatus are independent of the horizontal direction of the wind.

3. Statement of the problem

Perturbations of the airflow over mountains make it difficult to obtain exact measurements of rainfall in such regions (Hayes and Kittredge 1949). But even in laminar airflow supposed to follow the configuration of the mountain, the measurements cannot be expected to be as precise as those in a plain, because in an airstream with an upward directed component, raindrops of a certain size may move horizontally or upward, depending on the wind speed. Lütschg (1937) has employed a raingauge which can be exposed so that its catching area is directed to the wind. Brooks (1954), however, shows the impracticability of raingauges that are kept pointed into the wind.

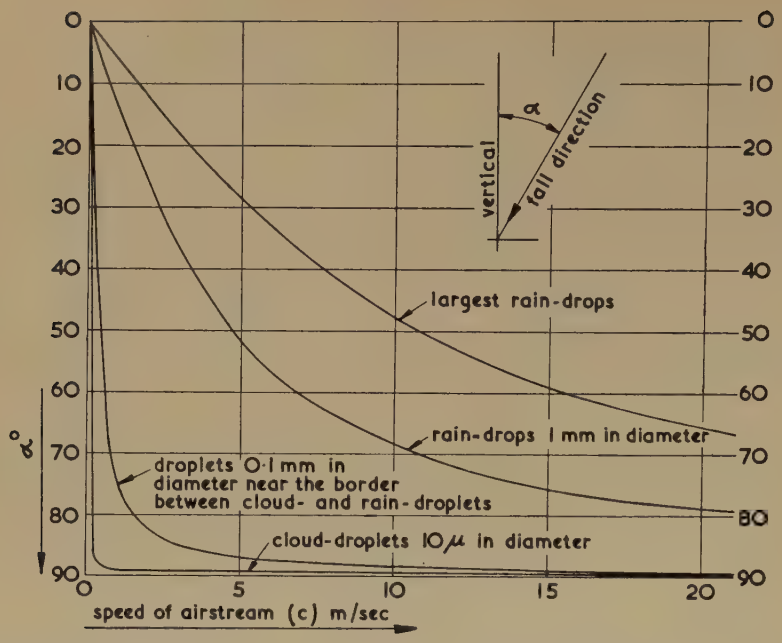

Figure 6. The graph represents the angle (α) at which the largest raindrops, drops of 1 mm, droplets of 0·1 mm, and cloud droplets 10 μ in diameter fall in a horizontal, laminar airstream of speed c.

The object of the fog-catcher is to catch fog- or cloud-droplets which are supposed to be blown against the apparatus in a horizontal airstream, and which cannot enter the standard raingauge. When the airstream is inclined at an angle to the horizontal, the catching area will not change much even though the angle be increased to say 20°, but in this case, cloud droplets can enter the standard raingauge. Another problem is to measure fog precipitation during rainfall. It would appear that a solution to this problem could be found if an anemograph and a drop-size recorder were applied. The instrument described by Kopp (1954) may be useful for this purpose. It should, however, be kept pointed into the wind, otherwise the apparatus will generally not be able to catch raindrops as can be seen from the graphs of Figs. 5 and 6.

Fig. 5 shows the angle α, the deviation from the vertical at which waterdrops of different sizes fall in a horizontal, laminar airstream of speed c, assuming them to have attained the speed of the airstream in the horizontal. The terminal velocity of fall for raindrops, determined by Gunn and Kinzer (1949) and for cloud droplets, from Stokes's law, is used to calculate the graphs. The diagram of Fig. 6 shows the angle α as a function of the wind speed for the largest raindrops, for drops of 1 mm, for droplets of 0·1 mm, and for cloud-droplets of 10 μ diameter.

From these graphs it can be seen that the smallest raindrops travel almost horizontally if the wind speed exceeds only some metres per second. The graphs indicate also the difficulty of distinguishing between fog-precipitation and drizzle. In order to find a solution, the following assumption is made : If the trace of an automatic raingauge without fog-catcher is fairly straight during the time interval under consideration, then 1·0 mm/hr of precipitation can be regarded as the border between drizzle and light rain.

A similar definition is given to distinguish between fog precipitation and drizzle. The limiting rate is assumed to be 0·1 mm/hr. This definition is applied for the evaluation of the records (see § 5).

4. The situation on table mountain

In February 1954 two ordinary raingauges (German type with 200 cm^2 catching area) and two South African recording raingauges with a catching area of approximately 127 cm^2 (Nagel 1954), one of each type equipped with a fog-catcher, were installed on Table Mountain some 50 m east of the summit station of the Cableway and about the same distance from the northern precipice of the mountain. Approximately 10 m from the instruments there are large rocks and other obstacles which are normally considered to be a good windbreak to shield the raingauge from excessive wind and thus give more reliable measurements of the rainfall, but they undoubtedly have a disturbing effect on airflow near the ground and will, therefore, affect the fog measurements. To ensure the services of an observer, a place more suitable for the measurements could not be chosen. This was most unfortunate, also because the Table Cloth appears more frequently and is more dense in the middle of the plateau than at the rim. The readings of the two ordinary instruments were taken at 08 hr from 1 March, 1954 to 28 February, 1955 by Mr. L. A. Vermaak, of the Cableway Company, who also attended to the recording instruments and estimated the wind.

From former wind records, taken with a Dines pressure tube anemometer at this station during the period 1935 to 1943, the average wind speed was determined to be 13 m/sec at the times that the Table Cloth covered the mountain. The mean height of the condensation level is supposed to be 300 m below the plateau, so that on the average the Table Cloth comes in direct contact with the upper portion of the mountain over an area of approximately 6 km^2 (about 2 km in a north-south direction and 3 km in an east-west direction).

During rainfall the upper portion of Table Mountain is generally covered by clouds. The station, therefore, is then within the cloud. According to several observers the average height of the base of rain-clouds on Table Mountain is about the same as that of the Table Cloth, the mean top of which is estimated to be 250 m above the plateau.

5. Evaluation and results

There are some differences between the measurements with the ordinary and with the recording instruments, but on the whole the agreement is satisfactory. For the calculations the indications of the recording instruments are used, but are supplemented by the readings of the ordinary instruments for several days, as some charts were blown away in strong winds and lost. The chosen days, given in Table 1, show how the different combinations of precipitation are evaluated, so as to find the amount of fog precipitation which is not measured by the standard raingauge.

In the four columns ' Precipitation ' of Table 1, there are given the observers' readings of the ordinary instruments, G_1 (without) and G_2 (with the fog-catcher), and the amounts

TABLE 1. Precipitation measured by the different gauges.

Item	Date (Observations at 08 hr) 1954	Precipitation (mm) Measured G_1	G_2	Recorded SA_1	SA_2	Wind Direction	Speed (m/sec)	Fog precipitation (mm)	Duration of fog precipitation (hours)
i	27-28 Mar.	16·3	29·1	16·2	31·9	S	8	—	—
ii	21-22 Apr.	0·3	36·0	0·2	37·5	SE	12	37·3	10·7
iii	5- 6 Aug.	3·7	88·8	3·6	81·9	WNW	15	55·9	18·3

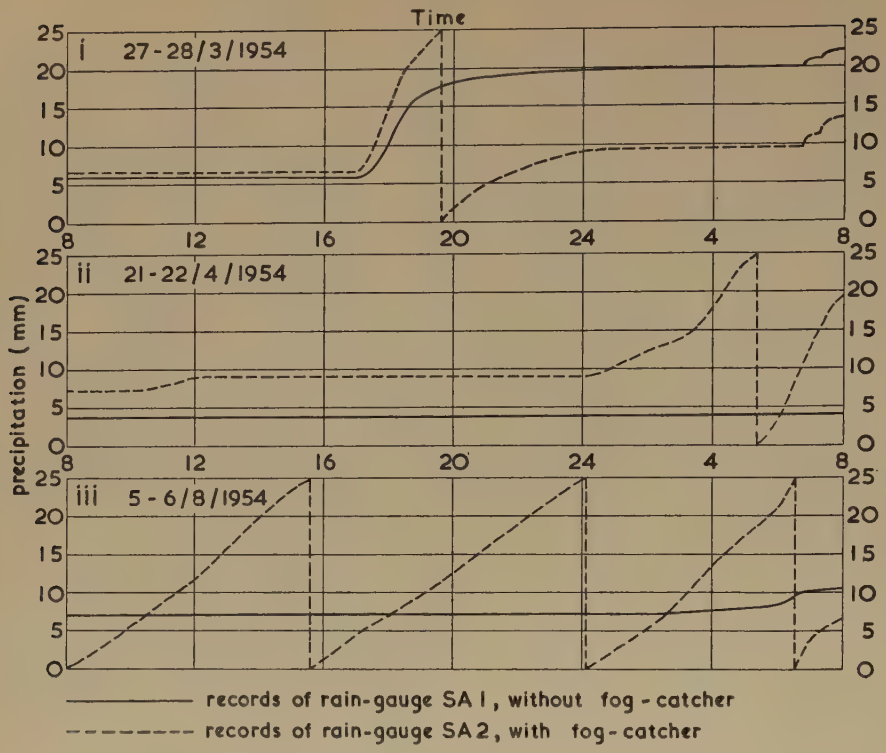

Figure 7. Fog precipitation and rainfall records on Table Mountain.

of precipitation obtained from the records of the automatic instruments, SA_1 (without) and SA_2 (with the fog-catcher). The explanation of the last two columns is given below.

For each day two automatic records are represented in Fig. 7, the full lines referring to the records of the instrument SA_1 without the fog-catcher and the dashed lines to those of the instrument SA_2 with the fog-catcher.

(i) Item i (Table 1) is an example of days with rain. Figure 7, i shows the two records of the automatic instruments. From the traces it can be seen that the precipitation started and ended at the same hour. The two records are parallel to the base line during equal time intervals. The amplitudes of the two records differ from each other owing to the increased catching area of the instrument SA_2. Fog precipitation is most likely to have occurred during the rainfall on this day but could not be determined.

(ii) The trace of the instrument SA_1 in Fig. 7, ii (full line) is almost parallel to the base line. The small amount of precipitation, 0·2 mm, indicated by this record is probably due to eddies which forced some of the cloud droplets into the funnel of the instrument. The dashed line, which represents the record of the instrument SA_2, shows spells of precipitation from 0930 to 1220 hr and from 0010 to 0800 hr, but during the remaining hours of the day the trace is parallel to the base line. The two spells show a total of 37·5 mm of precipitation during 10·7 hr (see diagram ii of Fig. 7).
The small amount of precipitation gathered by the instrument SA_1, although it was not caused by drizzle but by cloud droplets which entered the raingauge, is nevertheless subtracted from the amount collected with the instrument SA_2, because 0·2 mm or so of precipitation would have been measured by any raingauge. Consequently, 37·3 mm of fog precipitation during 10·7 hr is given in the last two columns of Table 1, ii.

(iii) The records in Fig. 7, iii show fog precipitation from 0800 hr to 0220 hr, then drizzle, thereafter rainfall, and finally again drizzle. With the definition given in § 3, fog precipitation on this day is evaluated to be 55·9 mm, occurring during 18·3 hr.

The explanations given above will be sufficient to show how the records are evaluated to find the fog precipitation. Table 2 shows the results obtained.

TABLE 2. MONTHLY VALUES OF RAINFALL, FOG PRECIPITATION, DURATION AND MEAN INTENSITY From 1 March 1954 to 28 February 1955 on Table Mountain, Station 20/717, 3,500 ft (1067 m) a.s.l.

Month	Mean rainfall over 13 years mm	Rainfall mm	Duration of rainfall hours	Mean rainfall intensity mm/hr	Fog precipitation mm	Duration of fog precipitation hours	Mean fog precipitation intensity mm/hr
Apr. 1954	137	146	84	1·74	281	73	3·85
May 1954	171	426	209	2·04	242	70	3·46
June 1954	157	169	94	1·80	233	68	3·43
July 1954	232	315	202	1·56	292	74	3·95
Aug. 1954	158	172	88	1·95	304	76	4·00
Sept. 1954	141	135	97	1·40	238	68	3·50
Winter	996	1363	774	1·75	1590	429	3·70
Oct. 1954	100	109	64	1·70	274	72	3·80
Nov. 1954	65	62	37	1·68	267	70	3·81
Dec. 1954	40	73	45	1·62	279	74	3·77
Jan. 1955	63	44	26	1·70	398	103	3·86
Feb. 1955	38	227	84	2·70	269	70	3·84
March 1954	64	62	29	2·14	217	58	3·74
Summer	370	577	285	2·03	1704	447	3·80
Year	1366	1940	1059	1·84	3294	876	3·75

6. DISCUSSION OF THE RESULTS

During the measuring period of one year there were 116 days without any precipitation, 126 days with rainfall, 123 days with fog precipitation from the Table Cloth in the absence of rain. Fog precipitation was determined also on 89 of the 126 days with rainfall.

Fog precipitation from the Table Cloth takes place mostly in summer, but from rain clouds when rain is not actually falling more often during winter than in summer. Fog precipitation during rainfall within the cloud could not be detected and is not given, although it undoubtedly occurred. For this reason the fog precipitation, given in Table 2, will probably be an underestimate, especially in winter. Since the measurements were made too far away from the middle of the plateau, as has been mentioned in § 4, there will be further underestimation, especially in summer. The intensity of fog precipitation is not much greater from the Table Cloth than from rain clouds, the average being 3·75 mm/hr.

The intensity of fog precipitation compared with that of rainfall is surprisingly high. This intensity is a function of the liquid-water content of the cloud and the speed with which it moves. The conditions are found to be such that generally clouds drift so fast over Table Mountain that the intensity of fog precipitation, measured and evaluated as explained above, is approximately twice that of the mean rainfall.

Following Schumann (1943), it is found theoretically that, if a laminar airflow is considered, and water from the Table Cloth is not deposited on the slopes, the air on the plateau could contain on the average, approximately 1 g/m^3 of water, which is about the same as in rain clouds. Since the mean wind speed was found to be 13 m/sec, the amount of liquid water which passes through 1 m^2 on the mountain is seen to be $46{\cdot}8 \times 10^3$ g/hr, equal to 46·8 mm/hr. This amount is about 12 times the value obtained experimentally. The question thus arises how this discrepancy is to be accounted for. It is explained as follows :

(i) The average base of the clouds may be estimated too low, so that the cloud would contain less than 1 g/m^3 of liquid water on the plateau.

(ii) It has been mentioned above that the fog measurements show a deficiency for the following reasons:
(a) the effective catching area of the gauze cylinder was too small,
(b) the instruments were situated near the precipice at the western end of the plateau where the cloud is less dense than in the middle of the mountain,
(c) a smooth airflow was obstructed by rocks,
(d) the mean velocity of the air at gauge height was probably less than the average wind speed of 13 m/sec measured at anemometer height.

(iii) The amount of water deposited by fog precipitation on the slopes of the mountain was not measured. Fast airflow over mountains must cause considerable perturbation (Corby 1954). As the slopes of Table Mountain are very rough, eddy motion will be formed and thus air from a somewhat higher layer will be directed downwards so that more cloud- and fog-droplets can be deposited on abundant vegetation than in the case of a laminar airflow. Therefore, the fog precipitation on the slopes might be greater than calculation for a wet-adiabatic ascent indicates. On the other hand, the plants may sift out such a great proportion of the water droplets that the air will reach the plateau with a smaller liquid-water content than that which is calculated theoretically.

7. Conclusions

An attempt was made to determine the fog precipitation on Table Mountain. However, measurements only on the plateau proved to be inadequate for the determination of the entire fog precipitation on the mountain. For further investigation it is intended to carry out experiments with larger gauze cylinders. The summit station should be shifted some 500 m to the middle of the plateau at a place where the airflow is not so much disturbed. At least a second station some 200 m lower on the slope is required. Each station should be supplied with a set of instruments consisting of : two recording raingauges, one of which is equipped with a fog-catcher, an anemograph, and a drop-size recorder. A method of identifying fog precipitation during rainfall and of distinguishing between fog precipitation and drizzle has been indicated.

Acknowledgments

The writer wishes to express his gratitude to Dr. T. E. W. Schumann, Director of the South African Weather Bureau, for his encouragement and advice in this work. He also appreciates the assistance of Mr. L. C. Hyde, manager of the Cableway Company, and Mr. L. A. Vermaak, who took the observations.

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551.556 : 551.46 : 532.59

An investigation into the generation of waves when the fetch of the wind is less than 100 miles

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Summary

Wave records taken at Lough Neagh by wave-pressure recorders and waves recorded over the Irish Sea by an airborne wave recorder are used to investigate the variation of wave height and wave period with fetch and wind speed, when the fetch is below a hundred miles. For values of fetch below 1 mi, these observations are supplemented by measurements taken at Staines Reservoir.

Empirical formulae are obtained for the maximum wave height, significant wave period, and spectral distribution of wave energy which reduce in the case of a large fetch to those already derived for waves in the open sea from weather ship observations. The formulae indicate that the steepness of the highest waves increases linearly with the wind speed and decreases with increasing fetch.

An expression is obtained for the mean square slope as defined by Cox and Munk (1954) and, under certain conditions, agreement is found with their results.

1. Introduction

Although a great deal of work has been done in investigating the generation of waves when the fetch of the wind is about 1 mi or less, and also when the fetch is very large (100 mi or more), there has been relatively little work done for intermediate values of fetch. The wave forecasting relations given by Sverdrup and Munk (1947) and later modified by Bretschneider (1952), cover fetches of the order 0 to 100 mi but they are based on very few observations in the range 10 to 100 mi. It was suggested (Darbyshire 1952, 1955) that wave characteristics no longer change with increasing fetch when the fetch exceeds 100 mi or so, and, if this is the case, waves must grow mainly when the fetch is less than 100 mi. The study of the generation of waves over this intermediate range of values of fetch must then be very important and although there is a limited amount of data available at present, it was thought worth while to investigate the subject to see how the results compare with those found for other values of fetch.

Work on waves acted on by winds over short fetches has been done by Johnson (1948) on Clear Lake, where the fetch varied up to 6 mi, and (1950) at Abbott's Lagoon where the fetch varied up to $\frac{3}{4}$ mi. More recently, Burling (1954, 1955), has investigated wave generation on Staines Reservoir where the maximum fetch was also $\frac{3}{4}$ mi. The U.S. Beach Erosion Board (1955) has studied wave generation over Lake Okeechobee, Florida, where the fetch varies up to 35 mi, but, as the depth is very small, about 1 ft for most of the fetch, the behaviour of waves on this lake cannot be regarded as typical.

In a previous paper, Darbyshire (1952), the results were described of experiments over Lough Neagh where the fetch varied up to 16 nautical miles and on the Irish Sea where the fetch varied up to 100 naut mi. Formulae were obtained giving the maximum wave height, significant wave period (mean period of highest third of waves) and the spectral distribution of wave energy. These formulae reduced in the case of long fetch to those derived in the same paper for waves recorded at Perranporth, Cornwall, which had been acted upon by the wind for long distances. A later paper (Darbyshire 1955), showed, however, that the wave generating conditions near the coast, as at Perranporth, are not typical of conditions in the open ocean and different formulae were derived for this case based on wave records taken by a ship-borne wave recorder (Tucker 1952) on the

O.W.S. *Weather Explorer*. The conditions of wave generation on Lough Neagh, where there are no appreciable currents and the depth of 40 to 60 ft is large compared with the wavelength, would be expected to correspond more to the open sea conditions and it was decided to re-examine the Lough Neagh data in the light of the results found for the open sea. Although conditions in the Irish Sea cannot be regarded as open sea conditions, the difference between open sea and coastal conditions was shown to be least when the wind was force 6 to 7, as it was during the experiments on this sea, so these results were still used. For the case of very short fetch, the data from these two experiments have been supplemented by measurements of wave records taken at Staines Reservoir which were kindly lent by Dr. Burling.

2. Recording at Lough Neagh

Fig. 1 is a map of Lough Neagh showing the position of the wave recorders and of the R.A.F. station at Aldergrove where the wind measurements were taken. Two wave-pressure recorders were used, both of a pneumatic type, using a rubber pipe. One was placed at a depth of 5 ft at a position on the north-east corner of the lake near Antrim. The other recorder was portable and could be used from a boat; it was placed in turn at a depth of 2 ft at a number of positions on the lake as shown on the map. The fixed recorder was operated continuously from 10.4.49 to 26.4.49 and the portable recorder used at frequent intervals during this period. The depth of recording made it difficult to estimate accurately the surface height from the wave pressure records when the wave period was less than 2·25 sec for the fixed recorder and 1·75 sec for the portable recorder. This meant, with the wind speeds which prevailed during the investigation, that wave data were only considered when the fetch of the wind was more than 9 mi with the fixed recorder and more than 2 mi for the portable recorder.

Figure 1. Map of Lough Neagh.

The records from the fixed wave recorder were investigated first. The wind speed and direction were obtained every half-hour from the Dines anemometer records and meaned over 3-hr intervals. The winds were measured at 12 m height. The mean fetch was obtained in all cases from the mean wind direction by considering the shape of the lake. The maximum wave height and the significant wave period were obtained from the wave records by measuring over a five-minute length every hour. These values were meaned over the same three-hour interval as the wind data. The wind speeds were multiplied by $\frac{3}{2}$ to make them comparable with the gradient wind-speed values used in the open sea formulae, which were taken to be $\frac{3}{2}$ times the surface-wind speed as measured at the masthead. The wave heights are plotted against the square of the wind speed in Fig. 2 and the periods against the square root of the wind speed in Fig. 3. The graph

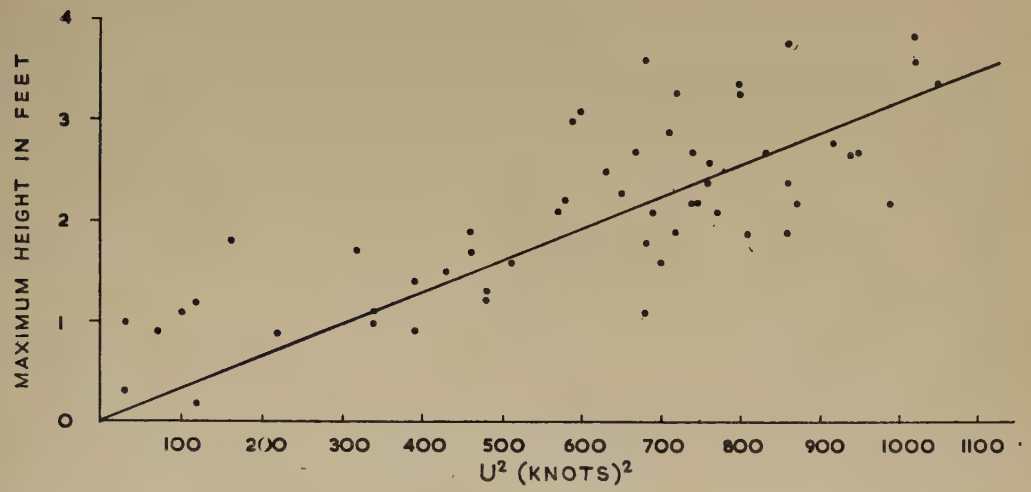

Figure 2. Graph of maximum wave height against the square of the wind speed.

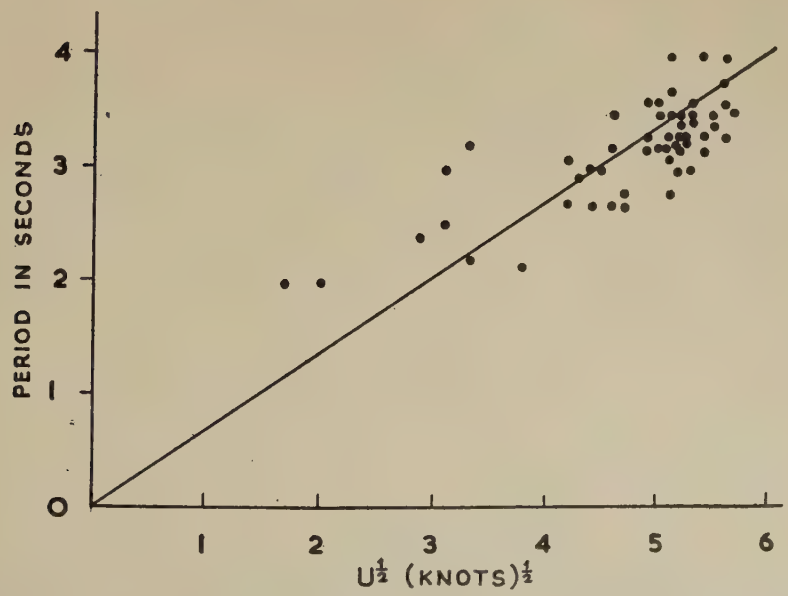

Figure 3. Graph of the significant wave period against the square root of the wind speed.

of the maximum wave heights indicates a relationship :

$$H_m\,(\text{ft}) = 0{\cdot}0032\,U^2\,(\text{kt}^2)$$

and the periods :

$$T_s\,(\text{sec}) = 0{\cdot}67\,U^{\frac{1}{2}}\,(\text{kt}^{\frac{1}{2}})$$

Where H_m is the maximum wave height, T_s the significant wave period, and U is $\frac{3}{2}$ times the wind speed. The mean fetch over all the observations was 11 mi. The corresponding open sea formulae are :

$$H_m = 0{\cdot}0076\,U^2, \quad T_s = 1{\cdot}64\,U^{\frac{1}{2}}$$

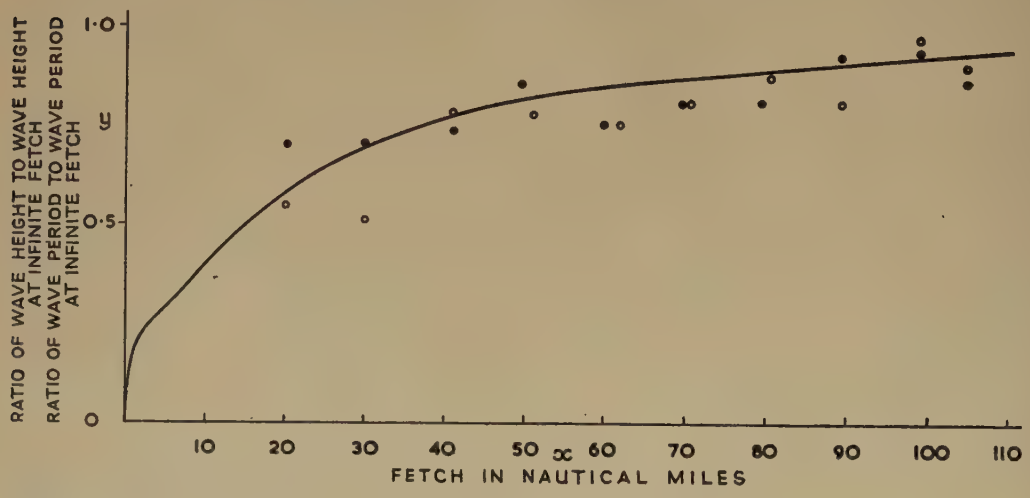

Figure 4. Plot against fetch of the ratio of wave characteristics to those for large fetch for the same wind speed : 20 to 100 mi.

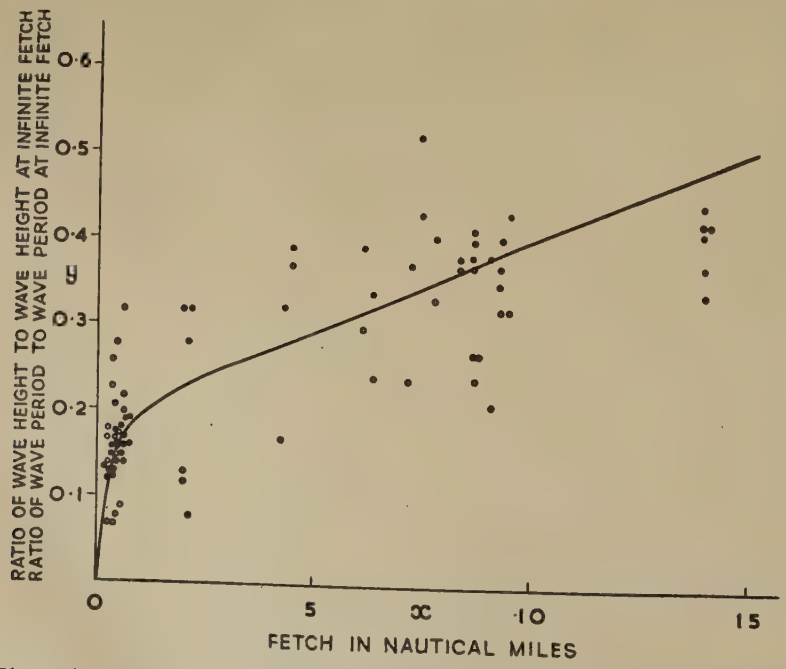

Figure 5. Plot against fetch of the ratio of wave characteristics to those for large fetch for the same wind speed : 0 to 14 mi.

The height and period for 11 mi fetch bear the same ratio, 0·41, to the corresponding values at large fetch. This is also true for larger fetches as shown by the Irish Sea results in Fig. 4, where the ratios of both the wave period (closed circles) and the wave height (open circles) to the large fetch values are plotted against the fetch. It is clear that both sets of points fall nearly on the same curve.

Examples where the fetch was less than 9 mi were investigated by using the results of the portable wave recorder, with which the fetch varied from 2 to 13 mi, and the measurements at Staines Reservoir, where the fetch varied up to $\frac{3}{4}$ mi. These results are shown in Fig. 5, where the ratio of the observed periods (closed circles) and observed heights (open circles) to the values at large fetch are plotted against the fetch. There is a good deal of scatter but the two sets of points appear to coincide after 8 mi fetch, while below this, for Lough Neagh, the periods tend to be too large and the heights too low. This could be due to the method of recording as the shorter-period waves tend to be much more attenuated, leaving a trace on the recorder of waves of longer period but smaller amplitude. These errors do not apply to the Staines Reservoir measurements, as a recorder of the actual surface profile was used (Tucker and Charnock 1954). In this case the ratios for the heights and periods again tend to coincide.

In general, it appears than that if x is the fetch in nautical miles, and y is the ratio of height at fetch x to height at infinite fetch, which is the same as the ratio of period at fetch x to period at infinite fetch, then

$$H_m = 0{\cdot}0076\, y\, U^2 \qquad (1)$$

$$T_s = 1{\cdot}64\, y\, U^{\frac{1}{2}} \qquad (2)$$

y is a function of x which in a previous paper (Darbyshire 1952) was given as

$$y = 1 - \exp(-0{\cdot}23\, x^{\frac{1}{2}})$$

A better numerical approximation to the points shown on Figs. 4 and 5 can be found by fitting the ratio of two polynomials so that

$$y = (x^3 + 3\,x^2 + 65\,x)/(x^3 + 12\,x^2 + 260\,x + 80) \qquad (3)$$

The full curves shown in Figs. 4 and 5 correspond to this expression and, considering the data as a whole, there is fairly good agreement for fetches varying from $\frac{1}{4}$ mi to 100 mi.

The steepness of the highest waves is given by $H_m/\{(g/2\pi)\, T_s^2\}$ and is therefore inversely proportional to y, increasing as the fetch decreases. This point was mentioned in the previous paper (Darbyshire 1955) where it was shown that the steepness of the highest waves at Staines Reservoir was on the average about six times, and at Lough Neagh four times, that measured in the open sea.

3. The wave spectrum

For a large fetch in the open sea, the spectrum was given (1955) by :

$$H_T = 0{\cdot}0036\,(U^{\frac{1}{2}} - 0{\cdot}43T)\, T^{5/2}$$

$$H_T = 0 \text{ when } U^{\frac{1}{2}} < 0{\cdot}43\, T$$

H_T is defined by $E_T = \frac{1}{8}\, g\, \rho\, H_T^2$ where E_T is the wave energy per unit area contained by waves with periods between $T - \frac{1}{2}$ and $T + \frac{1}{2}$ sec, ρ is the density of water, and g

the acceleration of gravity. If E is the total wave energy per unit area, and if H is the height of a simple sine wave having the same energy as the complicated wave pattern, then

$$E = \tfrac{1}{8} g \rho H^2$$

Now $$E = \int_0^\infty E_T \, dT, \quad \text{so} \quad \tfrac{1}{8} g \rho H^2 = \int_0^\infty E_T \, dT = \tfrac{1}{8} g \rho \int_0^\infty H_T^2 \, dT$$

so that $$H^2 = \int_0^\infty H_T^2 \, dT$$

It was shown in a previous paper (Darbyshire 1952) that $H = \frac{1}{2} H_m$ and thus for short fetches, the spectrum must be such that

$$\left\{ \int_0^\infty H_T^2 \, dt \right\}^{\frac{1}{2}} = 0{\cdot}0038 \, y \, U^2$$

Also, as mentioned in the 1952 paper, examination of a number of wave records has shown that the significant period corresponds very closely with the period of maximum amplitude in the spectrum, so that T_s is the value of T for which H_T is a maximum. The simplest formula which satisfies these conditions is :

$$\left.\begin{aligned} H_T &= 0{\cdot}0036 \, y^{\frac{1}{2}} \, (U^{\frac{1}{2}} - 0{\cdot}43 T/y) \, (T/y)^{5/2} \\ H_T &= 0 \quad \text{when} \quad U^{\frac{1}{2}} < 0{\cdot}43 T/y \end{aligned}\right\} \qquad . \quad . \quad (4)$$

Some examples of the use of this equation are given in Fig. 6. $H_T/2^{\frac{1}{2}}$ corresponds to a half-second interval of wave period, $T - \frac{1}{4}$ to $T + \frac{1}{4}$ sec. The fetch in the examples varies from 5 to 14 mi and U from 12 to 27 kt. The formula appears to give at least an approximation to the spectral distribution, although with very short periods in the case of very short fetch it may not be so accurate as it tends to show the energy diminishing more rapidly with decreasing period than was found by Burling. Burling found that for short

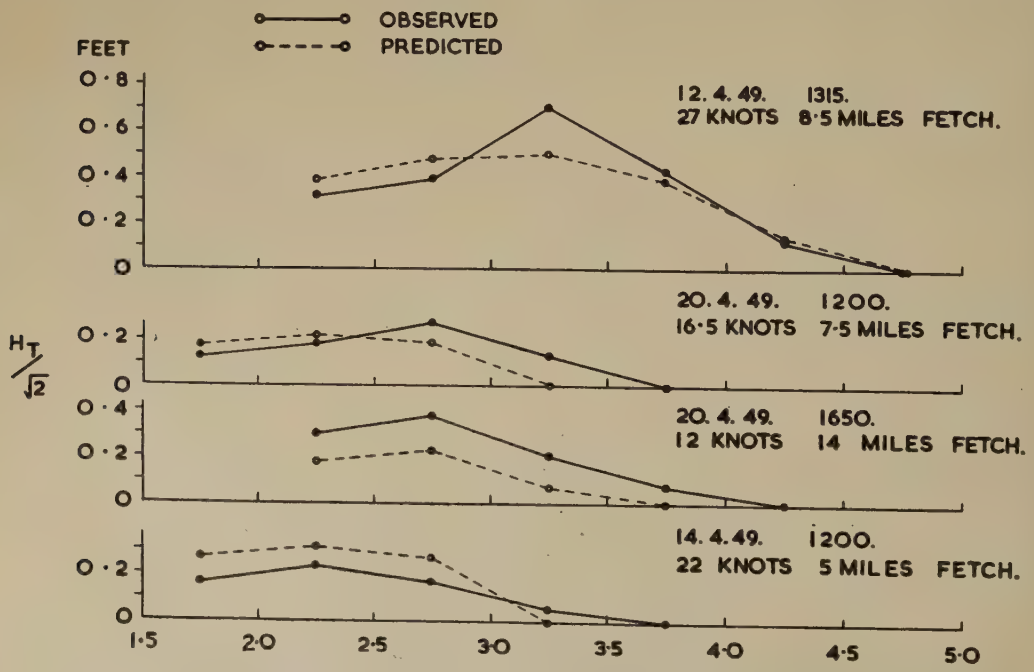

Figure 6. Predicted and observed wave spectra.

periods (or high frequencies), the energy per unit frequency range E_f varied inversely with fifth power of the frequency, whereas Eq. (4) suggests variation with the inverse seventh power of the frequency, for

$$E_T \propto y\,(U^{\frac{1}{2}} - 0{\cdot}43T/y)^2\,(T/y)^5$$
$$\propto (U^{\frac{1}{2}} - k/f)^2/f^5 \quad \text{for a given value of } y$$

Now $E_T\,dT = E_f\,df$ and therefore

$$E_f = E_T\,dT/df \propto (U^{\frac{1}{2}} - k/f)^2\,(1/f^5)\,dT/df$$
$$\propto (U^{\frac{1}{2}} - k/f)^2\,(1/f^7)$$

or when f is large, $E_f \propto U/f^7$

4. Mean-square slope

Cox and Munk (1954) define a parameter called the mean-square slope which is given by $\sigma^2 = \int_0^\infty \left(\frac{4\pi^2}{\lambda^2}\right) H_T{}^2\,dT$. From sun-glitter experiments near Hawaii, they derived the empirical expression $\sigma^2 = 5{\cdot}12 \times 10^{-5}\,W \pm 0{\cdot}004$ where W is the surface wind speed in cm/sec. It is possible, however, to find a quadratic expression, $\sigma^2 = 3{\cdot}2 \times 10^{-8}\,W^2 + 0{\cdot}0178$, which fits their results equally well. If Eq. (4) is substituted in the integral above with attention to the units one obtains :

$$\sigma^2 = 9{\cdot}1 \times 10^{-10}\,U^2/y^2, \quad U \text{ gradient wind speed in cm/sec.}$$

or
$$\sigma^2 = 2{\cdot}04 \times 10^{-9}\,W^2/y^2 \quad \text{if the surface wind } W \text{ is used.}$$

These expressions agree with the one obtained from Cox and Munk's results if $1/y^2 = 15$ so that $y = 0{\cdot}26$, corresponding to a fetch of 4 mi. An examination of the positions of recording given in Cox and Munk's paper shows that their measurements were taken at points about 5 mi equidistant from three islands and there was complete protection from winds blowing in the open sea when the wind direction was between 305° and 150° as was the case for nearly all their measurements, so that the effective fetch was nearly always about 5 mi. The large value of σ^2 at zero wind speed was possibly due to the incidence of swell from the gaps between the islands.

Conclusions

It is possible to extend to waves acted on by the wind over a distance of $\frac{1}{4}$ to 100 mi, the formulae for the maximum wave height, period of maximum amplitude, and the spectral distribution of wave energy, which were obtained in the case of a large fetch in the open sea. The formulae indicate that the mean-square slope and the steepness of the highest waves increase with decreasing fetch, and agreement can be found with Cox and Munk's data.

Acknowledgments

The author wishes to thank the Director and staff of the National Institute of Oceanography for advice and assistance in preparing this paper. He also wishes to thank his former colleagues, Dr. R. W. Burling for the use of the Staines Reservoir records, and Mr. C. M. Hubbard who was responsible for most of the wave recording at Lough Neagh.

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A wind analysis for the northern Irish Sea

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SUMMARY

This paper presents frequency distributions of winds recorded at five stations in the northern Irish Sea, Holyhead, Point of Ayre, Liverpool (Bidston), Southport and Fleetwood, particular stress being laid on the strong or gale-force winds. Figures are published which may be used for estimating the highest hourly wind speed and maximum gust to be expected in a given number of years. The direction and time of these extreme winds are discussed. The frequencies of winds of different speeds are tabulated; the frequencies of gales and winds in excess of 40 kt are examined in detail.

The relationships between the direction and strength of the geostrophic, gradient and surface wind over the area have been closely examined. It is found that neither the geostrophic nor the gradient winds can be used to provide quantitative estimates of the surface winds either in speed or direction, on specific occasions, nor is the surface wind at one station closely related to the surface wind at another.

The variation of wind speed throughout the year is indicated.

1. INTRODUCTION

Meteorological literature seems to lack an up-to-date analysis of wind strength over the Irish Sea. Gold's paper (1936) covering the whole of the British Isles cannot analyse any area in detail, and Bilham (1938) is largely dependent on data published in *The weather of the British coasts* (1918) and *Admiralty Pilots*, which in turn are mainly based on non-instrumental observations. There is clearly room for a detailed analysis of the records obtained from Dines's pressure tube anemographs. An examination by Baxendell (1936) of the diurnal variation of wind at Southport is worthy of mention in this connection. An investigation of winds over the Irish Sea seems desirable in view of the maritime importance of the Mersey estuary and its approaches, and of the extension of storm-surge research to cover this area. It is important to have data relating to the frequencies of gale force and strong winds.

The following sections deal with the records from five stations, whose salient features are listed in Table 1. These are the only stations in the area with sufficiently long records. As the prevailing winds are westerly, instruments on the east coast of Ireland, even if records were available, would not be representative of conditions over the sea. It did not prove possible to extend the investigation south of Holyhead, for although records for St. Ann's Head do exist for ten years during the period under review, its records are not, owing to nearby cliffs and consequent eddies, comparable with those from the other stations.

TABLE 1. LIST OF STATIONS.

Station	Date of installation	Height above sea level ft	Height above ground ft	Effective height ft	Remarks
Holyhead	1920	68	43	35	Moved to Valley Sept. 1952
Point of Ayre	1936	70	40	35	
Liverpool (Bidston)	1928	262	64	39	
Southport	1897	60	42	33	1935-48
		63	45	36	1949 onwards
Fleetwood	1923	112	50	31	

The Fleetwood record is not quite so complete as the others, and its consideration was omitted from § 4 and § 5. Nor were the Southport data considered for § 5.

The period analysed was 1935 to 1954 inclusive, with the exception of Point of Ayre, where the record began in 1936.

2. Annual highest hourly wind speed and maximum gust

Frequencies of the annual highest hourly wind speed for each of the five stations were extracted and the theoretical distribution computed by the method due to Jenkinson (1955), who showed that the probability P that the annual maximum value of a meteorological element is less than a given value x can be expressed as $\exp\left\{-\left(1-\frac{x}{a}\right)^{1/k}\right\}$ where a and k are constants such that ak is positive. The parameters a and k can be computed from data of annual maxima for a given period of years as :

$$\sigma_1/\sigma_2 = 2^k \quad . \quad . \quad . \quad . \quad . \quad (1)$$

and

$$a = \sigma_1 \{(2k)! - (k!)^2\}^{-\frac{1}{2}} \quad . \quad . \quad . \quad . \quad (2)$$

where σ_1 is the standard deviation of annual maxima and σ_2 is the standard deviation of the greater members in pairs of annual maxima. Once the ratio σ_1/σ_2 has been evaluated from the data, the table for probable 10, 25, 50, 100 and 250 year maxima given in the original paper can be used.

The continuous curves of Figs. 1 to 5 show the theoretical probability curves of maximum hourly wind speed calculated by this method, with speed in knots plotted as abscissa and the parameter y as ordinate where

$$y = -\ln f(x) \quad . \quad . \quad . \quad . \quad . \quad (3)$$

and $f(x)$ is the average annual number of daily values greater than x.

The goodness of fit of these curves may be assessed by comparisons with the crude distribution plotted as dots. This crude probability has been derived using Hazen's frequency (1930). If a series of annual maxima are arranged in ascending order of magnitude X_1, X_2 . . . . . X_n, then the true cumulative frequency of the mth value of the series lies between $(m-1)/n$ and m/n, and

$$P = (2m-1)/2n \quad . \quad . \quad . \quad . \quad (4)$$

may be taken as compromise value.

Values of y corresponding to the values of $P = 1/2n$, $3/2n$ . . . . . $(2n-1)/2n$ were therefore plotted against the values of x in ascending magnitude.

It will be seen that in all cases the theoretical curve is an excellent fit to the data.

Comparison of Fig. 1 with Figs. 2 to 5 shows that for a given frequency, winds at Holyhead are 3 to 8 kt stronger than at the other stations. There is no significant difference between the distributions at Liverpool, Southport, Fleetwood and Point of Ayre. This has been illustrated by the plotting of the broken lines representing the mean curve of the four computations on each diagram. This curve has only been drawn where it differs from the station curve already plotted. It has also been drawn fully on the Holyhead graph to facilitate comparison there.

The annual maximum value which will be exceeded once in T years is determined from the relationship :

$$y = -\ln\ln\{T/(T-1)\}. \quad . \quad . \quad . \quad (5)$$

Values of wind speed (whether annual maxima or not) which will be exceeded once in T' years can also be determined graphically from the theoretical curve, as

$$y = \ln T' \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

and the corresponding wind speed can be read off.

A scale showing some values of T' appears on the right-hand side of Figs. 1 to 5. The curves have been extrapolated to a return period of 100 years, but as they are based on only 20 years' observation, these extremes should be treated with some reserve.

The annual maximum gusts were treated in a similar manner, and the results are graphed in Figs. 6-10. The curves for Holyhead and Liverpool are sufficiently distinct from each other and from the others, whilst the results from Point of Ayre, Fleetwood and Southport display only minor differences, and the mean curve of the three computations is shown as a broken line. This broken line, and a dash-dot line representing maximum gusts at Liverpool have also been plotted on Fig. 6 for comparison purposes. The double crossing of the Holyhead curve by the Liverpool curve is interesting, and its reality receives some confirmation from the 1922-54 curve for Liverpool which, although partly outside the period being considered in this review, was available for comparison. This would plot approximately half-way between the Holyhead and the present Liverpool curves throughout the complete range of values of y, indicating that the additional 13 years of record would tend to close the gap between Holyhead and Liverpool, rather than the gap between Liverpool and the other stations.

The physical explanation of these high gusts at Liverpool, as will be shown later, must be that almost all abnormally strong winds there occur with a wind direction of 270° or 280°. Winds from this direction blow approximately 4 mi over land before reaching the anemograph (at low water, more than 6 mi overland owing to drying sandbanks), a greater overland fetch than at any of the other stations for their most prevalent gale direction. This, together with its hill-top position, would make for a more turbulent airstream at Liverpool.

TABLE 2. HIGHEST HOURLY WIND SPEEDS AND MAXIMUM GUSTS.

Station	Highest hourly wind speed			Maximum gust			
	Direction (degrees)	Speed (kt)	Date	Terminal hour	Speed (kt)	Date	Time (GMT)
Holyhead	280	56	4 Oct. 1938	06	93	28 Feb. 1937	0400
Point of Ayre	240	56	22 Sept. 1945	05	78	6 Dec. 1936	0040
Southport	280	52	4 Oct. 1938	07	78	4 Oct. 1938	0700
Fleetwood	330	52	17 Dec. 1952	11	79	17 Dec. 1952	1055
Liverpool (Bidston)	270	56	4 Oct. 1938	07	88	29 Jan. 1938	0910

Table 2 shows the highest hourly wind speed and maximum gust reported from each station between 1935 and 1954. A wind speed of 52 kt occurred on two occasions in 1952 at Fleetwood. The one chosen for inclusion was the occasion associated with the highest gust.

One out of five of the highest hourly wind speeds and two of the maximum gusts occurred during the forenoon. This is interesting in view of Gold's findings (1936) that, in the much wider sample of records which he examined, maximum gusts never occurred during this period of the day. This point is elaborated in § 4.

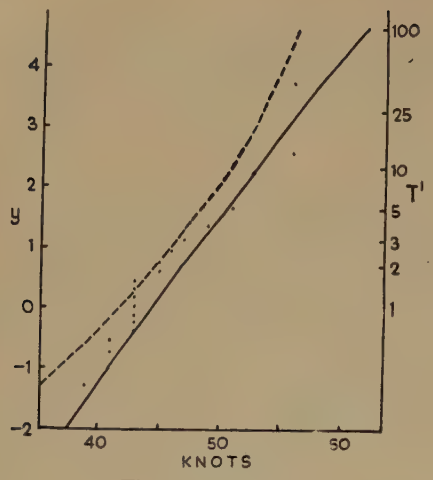

Figure 1. Holyhead.

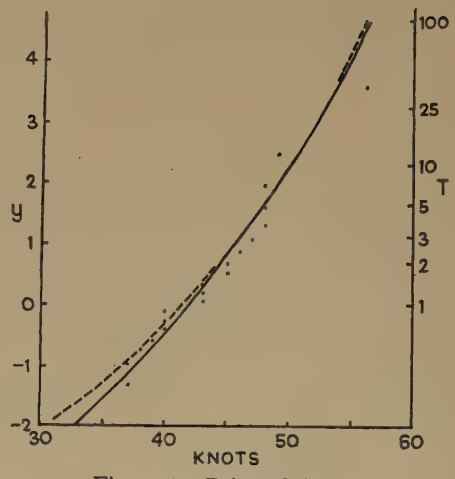

Figure 2. Point of Ayre.

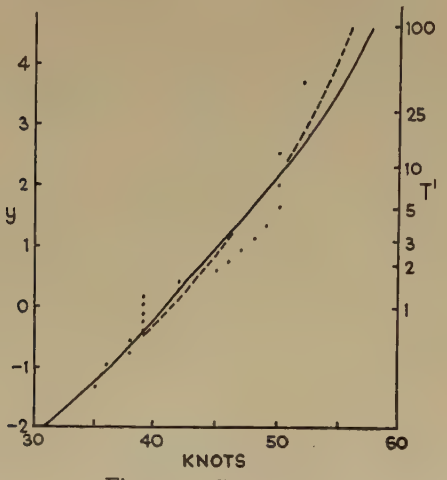

Figure 3. Fleetwood.

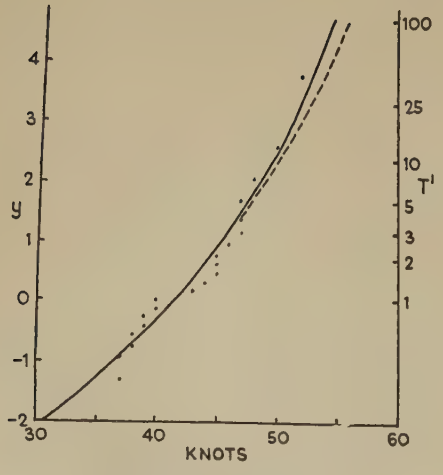

Figure 4. Southport.

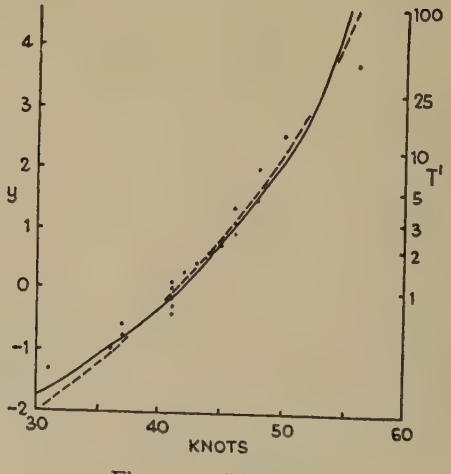

Figure 5. Liverpool.

Figures 1-5. Highest hourly wind speed in knots. —— Theoretical curve of station. – – – Mean theoretical curve of Point of Ayre, Fleetwood, Southport and Liverpool.

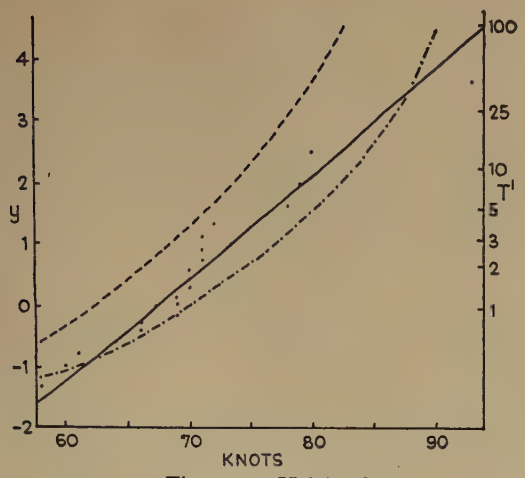

Figure 6. Holyhead.

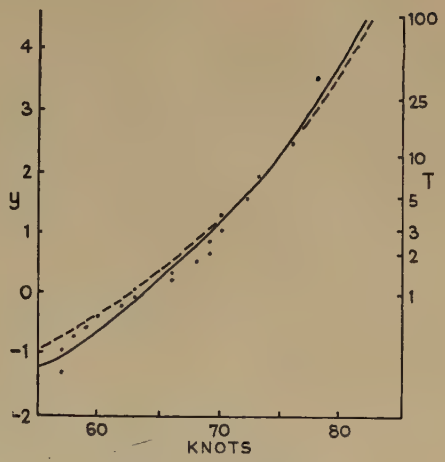

Figure 7. Point of Ayre.

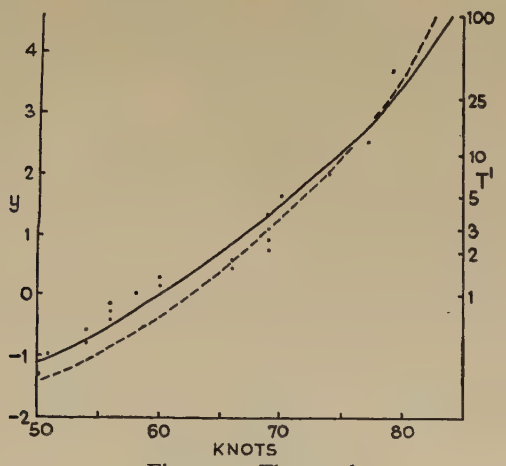

Figure 8. Fleetwood.

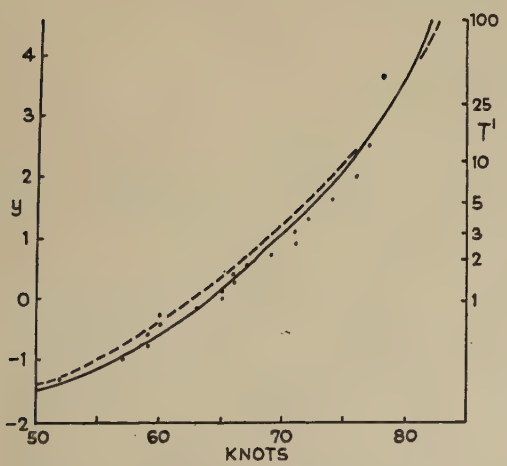

Figure 9. Southport.

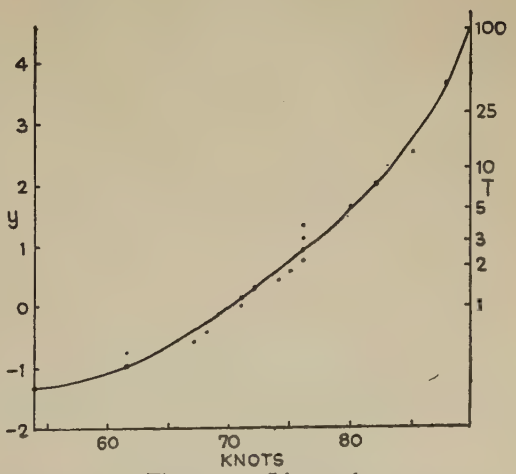

Figure 10. Liverpool.

Figures 6-10. Maximum gust in knots. —— Theoretical curve of station. - - - Mean theoretical curve of Point of Ayre, Fleetwood and Southport. — · — Liverpool curve on Holyhead diagram.

3. Distribution of winds of stated speeds

The average duration of winds of stated speeds at the various stations is given in Table 3. The number of days on which gales and strong winds occur is quoted, as is the percentage of the total period of 20 yr during which each anemograph was operating effectively. The Point of Ayre instrument was not erected until 1936 and so its averages are based on 19 yr instead of 20 yr. The instrument at Southport was out of order for only 215 hr during the 20 yr, a tenth of one per cent of the time.

TABLE 3. Distribution of winds of stated speeds

	Distribution of wind speeds							
	More than 33 kt		22-33 kt		11-21 kt	4-10 kt	0-3 kt	Percentage
Station	No. of days	Duration (hr)	No. of days	Duration (hr)	Duration (hr)	Duration (hr)	Duration (hr)	of reliable record
Holyhead	14·4	69	118·5	925	3968	2788	1016	97·8
Point of Ayre	13·5	63	129·5	1064	3534	2810	1295	93·3*
Southport	11·6	59	92·3	737	3084	3900	986	99·9
Fleetwood	9·3	46	78·0	540	3164	3858	1158	84·1
Liverpool (Bidston)	7·4	37	72·8	538	2970	4069	1152	98·5

* of 20 years, 1935-1954; 98·2 per cent of effective record from 1936 onwards.

The averages have been adjusted for any lost time so as to present durations applicable to a year without any instrumental breakdown. It was assumed that the periods of breakdown were at random during the year. In the one case where an appreciable correction had to be made, Fleetwood, a check was carried out to establish whether the periods of breakdown were evenly distributed between the calendar months, and this was found to be so.

Allowing for the fact that some gales will overlap into two days, the average duration of a gale, w, may be computed by successive approximation from the data of Table 3 using the formula

$$w = \frac{h}{d}\frac{24}{25 - w} \quad . \quad . \quad . \quad . \quad (7)$$

where h is the total number of hours of gale/year, and d is the number of days with gale.

This formula gives the average duration of a gale at all stations as 6·2 hr. The average numbers of gales/year are: Holyhead, 11·1; Point of Ayre, 10·2; Southport, 9·5; Fleetwood, 7·4; and Liverpool, 6·0.

It should be borne in mind that the term 'gale' is used throughout this paper to indicate a mean hourly anemometer wind speed exceeding 33 kt, whilst the official definition of a day of gale is a day on which a wind of force 8 or more (wind speed at 33 ft above ground exceeds 33 kt) has lasted for at least 10 min at any time during the 24 hr. It is not likely that this discrepancy will introduce any large frequency differences but its existence should be remembered.

Table 3 shows the considerably stronger winds to which the two more westerly stations are subject; gales and strong winds being almost twice as frequent at Holyhead and Point of Ayre as at Liverpool.

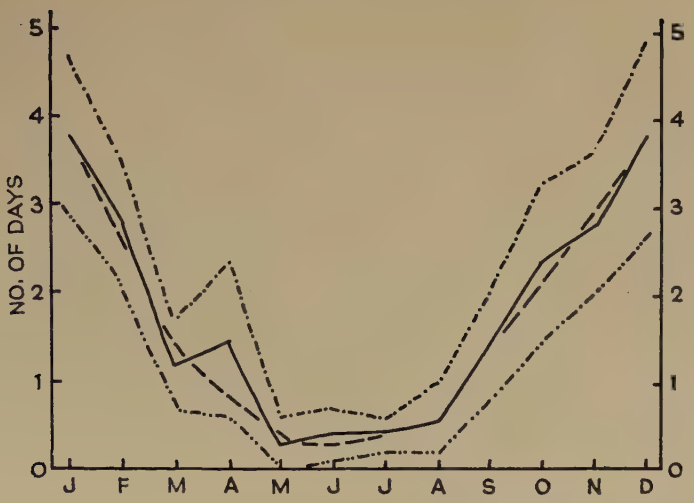

Figure 11. Annual variation in the number of days when the mean hourly wind speed at one or more stations exceeded 33 kt. Central continuous line is mean. Upper and lower lines depart from the mean value by twice the standard error of the monthly mean. Broken line is the projected smoothing discussed in text.

The average monthly frequencies of gale-force winds (34 kt or more) anywhere in the northern Irish Sea are given in Fig. 11. Some indication of the scatter of the individual monthly frequencies has been shown by plotting the values of mean plus and minus twice its standard error. With this in mind, the secondary maximum at April does not appear representative of long-period conditions, but is a peculiarity of this relatively small sample, and the freely drawn broken line is probably nearer to the true value of the mean.

4. Frequency of winds of 41 kt or more

The average frequencies of winds of 41 kt or more (Force 9) over the northern Irish Sea, as represented by their occurrence at one or more of the stations, Holyhead, Point of Ayre, Southport and Liverpool are shown in Fig. 12. The frequencies given are occasions, a gale being treated as a single occurrence whether it lasted over one or more civil days. The earliest date in the autumn accredited with a day of strong gale was 17 September. Strong gales took place on this date in three years out of the 20-yr period. The last date of occurrence in spring was 9 May.

Winds in excess of 40 kt occur on average on four occasions during the year over the Irish Sea, and the duration of gale associated with them averages 13½ hr at Holyhead, Point of Ayre and Southport, and 10 hr at Liverpool. The highest number of hours of gale associated with one strong gale was 52 hr at Point of Ayre in January 1940.

On 13 per cent of the occasions with winds of 41 kt or more, such winds were recorded at all four stations. On 9 per cent the strong gale was observed at three stations, and in six out of seven of these cases the three stations were Point of Ayre, Southport and Liverpool. It would seem that the exclusion of Holyhead in so many cases was due to more than chance. On 17 per cent of occasions such winds were measured at two stations, and in the remaining 61 per cent of dates the strong gale was recorded at only one station, the preponderance of these being at Holyhead.

The direction of the highest hourly wind speed associated with these periods was examined. At Holyhead the wind blew from between 250° and 320° on 76 per cent of

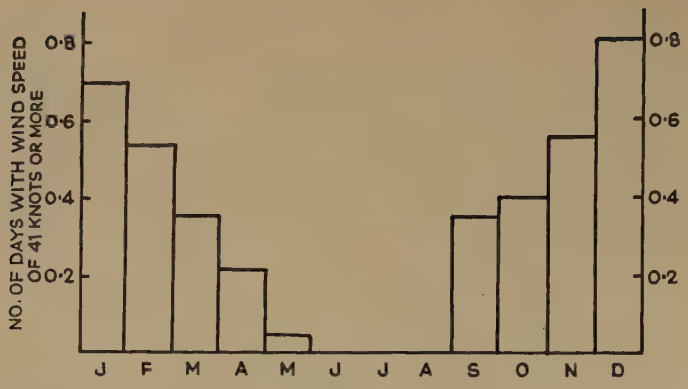

Figure 12. Average number of occasions with winds of 41 kt or more at one or more northern Irish Sea stations.

occasions, from approximately 80° on 10 per cent of occasions, from 160° on 5 per cent and from 210° on 7½ per cent of occasions.

At Point of Ayre the direction of strong gales was from 220° to 010° on 88 per cent of occasions (more than 50 per cent were from between 290° and 330°). Direction 230° just clears the north-western end of the mountainous part of the Isle of Man. Strong gales have also occurred with winds between 120° and 160°. The eastern end of the Manx massif bears 160° from Point of Ayre Lighthouse. On each occasion when strong gales from this quadrant occurred at Point of Ayre, they were not reported from any other Irish Sea station, the other anemographs being presumably on windward coasts.

At Southport the strong gales occurred with winds from between 230° and 300° on every occasion except one when the wind direction was 340°.

At Liverpool the strong gales were always from between 240° and 300°, and from between 270° and 300° on 92 per cent of occasions.

Gold (1936) using a number of anemograms, relating mainly to gusts in excess of 87 kt (100 mi/hr^{-1}), shows that such high gusts all occur during the afternoon or night, none in the forenoon. In order to test whether this conclusion could be extended to the Irish Sea area and to both the highest hourly wind speed and the highest gust associated with winds in excess of 40 kt, the terminal hours of highest hourly wind speeds and the times of highest associated gusts were tabulated. Although in each case there was some irregular variation, as might be expected from a small sample (145 cases) divided in 24 groups, significance tests based on χ^2 showed that such variation as did occur was not incompatible with random occurrence. The irregular variation observed in no way suggested that Gold's result might apply to these data. Thus there is no evidence to show that, in the area now considered, maximum wind speed and highest gusts may not be expected with equal probability at all times of the day or night.

5. Relationship between geostrophic, gradient and surface winds

On all dates when the mean wind speed reached 41 kt or more at any of the four stations whose records were examined in § 4, the speed and direction of the geostrophic wind at a point central to the sea area between the Isle of Man and the Mersey estuary at 06 or 07 hr were estimated from the synoptic maps in the *Daily Weather Report*, by laying a geostrophic wind scale across adjacent 2-mb isobars on $1 : 10^7$ scale charts. From 1950 onwards, 4-mb isobars were used with $1 : 2 \times 10^7$ scale charts. The speed

so computed from the charts was also adjusted for the curvature of the isobars so as to be an estimate of the gradient wind, by use of the formula for cyclonic curvature :

$$v = r\omega \sin\phi \left\{(1 + 2V/r\omega \sin\phi)^{\frac{1}{2}} - 1\right\} \quad . \quad . \quad . \quad (8)$$

where v is the gradient wind speed, V is the geostrophic wind speed as read from the isobars, ω the speed of rotation of the earth ($0{\cdot}262 \times \text{hr}^{-1}$), ϕ the latitude (taken as 54° for the Irish Sea), $r = R \tan\theta$, where R is the radius of the earth and θ is the angle subtended at the centre of the earth by the radius of curvature of the isobars. For anti-cyclonic curvature all the signs of Eq. 8 require changing.

As a rule one reading was considered to be an adequate estimate of the wind, but on a few occasions when the isobars appeared to be unevenly drawn, and yet without any apparent synoptic reasons for such irregularity, two or more values for the mean distance of the isobars were taken, and the wind scale applied to the average of these.

The geostrophic and gradient winds thus calculated were compared both in speed and direction with the observed surface winds at these times on the relevant dates at Liverpool, Holyhead and Point of Ayre, read from the appropriate anemogram. Such readings were spot values, approximating to 10-min means. It is fully realized that the reading off of geostrophic winds from the maps of the *Daily Weather Report* is to some extent a subjective process. To eliminate as far as possible any bias which might be introduced all such reading off was completed before any figures for the measured winds were examined.

In this section the relevance of wind data to storm-surge research is being considered. In this connection, it was desired to know with what degree of accuracy spot readings of geostrophic and gradient winds represented the surface wind blowing in the sea area. As the areas under consideration in the storm-surge problem are large compared with the north-east Irish Sea considered here, precise definition of the point at which the isobaric distance was measured was not deemed of high importance, and hence one central observation of the geostrophic and gradient wind was compared with the actual surface winds at the three stations. The results of these comparions are detailed in Tables 4 to 7.

It was found that there was a significant variation at Liverpool in the relationship between the geostrophic and surface winds depending on whether they were of seaward or landward fetch, and thus, in addition to the mean values, these two general directions are shown separately in Table 4, and the speed relationships for all directions and for seaward fetch are indicated in Table 6.

TABLE 4. Relationships between geostrophic and surface-wind directions

	Mean backing of surface wind relative to upper	Standard deviation	Max. veer	Max. back	No. of cases
			(degrees)		
Geostrophic and Liverpool					
(a) All cases except anticyclonic curvature	26	26	30	110	92
(b) Geostrophic wind between 260° and 360° (seaward fetch)	18	21	30	80	63
(c) All other directions (landward fetch)	42	29	20	110	29
Geostrophic and Holyhead	26	22	20	80	89
Geostrophic and Point of Ayre – all cases except anticyclonic curvature	2	21	50	60	81

At Holyhead the curvature of the isobars (whether cyclonic or anticyclonic) appeared to have no effect on the direction relationship, but cases of anticyclonic curvature at Liverpool and Point of Ayre gave occasional anomalous amounts of backing or veering. There was not, however, a sufficient number of these cases for their examination to be fruitful. Accordingly, only cases of straight or cyclonic-curvature isobars were examined for these two stations.

TABLE 5. RELATIONSHIPS BETWEEN SURFACE-WIND DIRECTIONS

	Mean	Standard deviation	Max. veer	Max. back	No. of cases
			(degrees)		
Backing at Liverpool relative to Holyhead	0	24	60	80	99
Backing at Holyhead relative to Point of Ayre	23	24	40	80	91
Backing at Liverpool relative to Point of Ayre	23	29	70	100	91

TABLE 6. RELATIONSHIP BETWEEN GEOSTROPHIC, GRADIENT AND SURFACE-WIND SPEEDS

		Correlation coefficient	Upper wind mean (kt)	Upper wind S.D. (kt)	Surface wind mean (kt)	Surface wind S.D. (kt)	Ratio of surface to upper wind	S.E. of estimate of surface wind (kt)
Liverpool	Geostrophic and surface	+ 0·53	54·6	17·3	25·4	10·7	0·46	9·0
	Gradient and surface	+ 0·56	46·8	13·0	25·4	10·7	0·54	9·0
	Geostrophic and surface (seaward)	+ 0·60	55·0	15·1	29·4	9·2	0·53	7·4
	Gradient and surface (seaward)	+ 0·54	48·8	12·6	29·4	9·2	0·60	7·7
Holyhead	Geostrophic and surface	+ 0·50	54·6	17·3	27·6	9·8	0·51	8·5
	Gradient and surface	+ 0·39	46·8	13·0	27·6	9·8	0·59	9·0
Point of Ayre	Geostrophic and surface	+ 0·58	54·6	17·3	27·6	8·8	0·51	7·1
	Gradient and surface	+ 0·61	46·8	13·0	27·6	8·8	0·59	7·0

TABLE 7. RELATIONS BETWEEN SURFACE-WIND SPEEDS AT THE DIFFERENT STATIONS

	Correlation coefficient	First station mean (kt)	First station S.D. (kt)	Second station mean (kt)	Second station S.D. (kt)	S.E. of estimate of surface wind at second station (kt)
Holyhead – Liverpool	+ 0·68	27·6	9·8	25·4	10·7	7·2
Point of Ayre – Liverpool	+ 0·68	27·6	8·8	25·4	10·7	7·6
Holyhead – Point of Ayre	+ 0·62	27·6	9·8	27·6	8·8	7·0

The main conclusions to be drawn from these Tables are : (*a*) There is very considerable scatter in the amount of backing of the surface wind from the geostrophic direction (winds blowing along the isobars). The standard deviation (S.D.) with winds blowing from seaward is around 21° at all three places. The direction of the surface winds may not then be estimated from the isobars with any degree of accuracy.

Using a result derived by Bannon (1949) that the wind at 3,000 ft at Liverpool had a mean angular deviation from the isobars of about 7° from high pressure to low pressure, it would appear that at Liverpool and Holyhead between the height of the anemometers, 40 ft approximately, and 3,000 ft, the average veer with height is about 20°, but Point of Ayre presents an anomalous result.

(*b*) There is slightly more scatter when surface winds at one point are compared with surface winds at another.

(*c*) The correlation between the geostrophic wind and the surface-wind speeds at all stations is of the order of + 0·55, and that between the gradient-wind speed and the surface-wind speed is of the order of + 0·52. It is puzzling to comprehend why the correction, although based on sound physical reasoning, does not effect *any* improvement. It is clear, however, that neither the geostrophic nor the gradient-wind speeds can be used to give good estimates of the surface-wind speed. In fact, the S.D. of the surface wind varies from 8·8 to 10·7 kt, whilst the standard error of the estimates of the surface wind from the geostrophic or gradient wind varies between 7·0 kt and 9·0 kt, so that the use of a regression equation is not a great improvement on estimation by mean values.

(*d*) The correlation between the surface-wind speeds at any two stations is of the order of + 0·66. The observed wind speed at one station is, therefore, not a very good guide to quantitative estimates of conditions over the area as a whole, or at another station.

(*e*) At Holyhead and Point of Ayre (and at Liverpool, when only directions from seaward are considered) the surface wind is approximately half the speed of the geostrophic wind and 6/10ths of the gradient wind speed.

In this connection the exposures of the three stations here examined approximate to conditions found at Stornoway by Marshall (1954). The ratios of the surface to the geostrophic-wind speeds of Table 6 are very close to that computed by Marshall for Stornoway (0·48). The mean backing of Table 5 also compares more closely with that for Stornoway than with that for the more exposed Scillies and Bell Rock, or for the more sheltered Kew.

Durst (1949) found that at Gorleston, the speed of the onshore surface winds was 4/5ths of the geostrophic wind, and the offshore surface winds were only 1/3rd as strong as the geostrophic wind. None of the Irish Sea stations has an exposure even approximating to this. At Liverpool with offshore winds, the ratio is about 1/3rd, but no station in the Irish Sea, here considered, has a ratio with onshore winds even approaching 4/5ths.

Some stress has been laid on the relationship between surface-and upper-wind conditions in this area, and on the errors likely to be involved when estimating one from the other, because this is a very relevant factor in storm-surge research, such as is being undertaken at the Tidal Institute. The sole purpose of this paper being to present facts, it is undesirable to discuss these points further; such discussion as has taken place being only to draw attention to the salient features.

6. Mean hourly wind speed

An indication of the variation of the wind speed throughout the year is given in Table 8 which lists the monthly mean hourly wind speed at Liverpool for the period 1929-54.

TABLE 8. MEAN HOURLY WIND SPEED (kt) AT LIVERPOOL, 1929-54

J	F	M	A	M	J	J	A	S	O	N	D	Year
12·2	11·7	10·0	11·6	8·8	8·6	9·2	8·6	9·8	11·3	10·8	12·2	10·3

These figures do not bear out the popular view that March is a windy month. Scrutiny of the individual monthly means shows that some degree of persistence between adjacent months exists. Months more windy than normal tend to be followed by similar months and vice versa. The autocorrelation coefficient is + 0·36, but this system breaks down between December and January and between January and February, where no persistence of type is displayed.

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A contribution to absolute pyrheliometry

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SUMMARY

The results are presented of comparisons made in Southern Africa, during the years 1951-1954, between pyrheliometers of the Ångström type which had been standardized at Davos (Switzerland), Leopoldville (Belgian Congo), Pretoria (South Africa) and Stockholm (Sweden). A section is also devoted to an investigation into the relationship between the original Ångström scale and that embodied in the Abbot pattern of actinometer of the Smithsonian Institution, Washington. This latter series of comparisons was made at Pretoria (altitude 1370 m), under normal conditions of cloudless sky with reasonably steady atmospheric transmission.

The programme was extended to include a comparison between specimens of these two sub-standard radiometers using a controlled laboratory source of radiation. In these conditions, the radiometers were found to agree to within one-half per cent. This is further evidence of the small divergence (first pointed out by Guild in 1937) of the scale originated long ago by Knut Ångström, from the true standard of radiometry.

1. INTRODUCTION

The international standardization of a radiation scale for meteorological purposes is still awaited. Of the two acceptable reference scales, viz. the classical ones of Ångström and of the Smithsonian Institution (1913 revision), it is believed that the former diverges less markedly from the true absolute scale of radiation than the latter, which has an acknowledged error now assessed at 2·5 per cent. Also, the Ångström scale either in its original uncorrected form or as Å + 3·5 per cent (a close approximation to the 1913 Smithsonian scale) is in widespread use. Indeed, a recent survey by the World Meteorological Organization (1954) revealed that 22 out of 43 countries were employing the Ångström scale basically or corrected by the above increment; at the remaining stations the Smithsonian scale as determined at Washington is in force.

The uncorrected Ångström scale was adopted in 1950 for the Union of South Africa and for the adjacent territories which depend upon the Weather Bureau for the provision of meteorological services.

2. THE EQUIPMENT

Three very old electrical compensation pyrheliometers were available in South Africa at the onset of the radiation programme. These were :

(a) No. 80 which had been constructed by Rose and certified (by K. Ångström) at Uppsala in June 1906,

(b) No. 146, of similar construction, certified at Uppsala in October 1911,

(c) No. 18494 constructed by the Cambridge Inst. Co. and certified (by Callendar) at London in 1914.

Pyrheliometers no. 80 and 146 were intended for use with a common mounting tube and had been employed at the Union Observatory in Johannesburg during the period October 1906-September 1915 to standardize the traces provided there by a Callendar sunshine recorder. There is no record of pyrheliometer no. 18494 (which had a different arrangement of diaphragms in its mounting tube from that of the Swedish instrument) ever having been used. There was also, in the Physics Department of the University

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of Stellenbosch, an Abbot silver-disc actinometer (no. s.1 43) which had recently been returned to Washington for modernizing (replacement of the original short tube by the present standard long one) and recertifying. The new certificate is dated January 1951. Through the kind co-operation of the Union Astronomer and the University of Stellenbosch, instruments nos. 80, 146 and s.1 43 were made available to the Weather Bureau for incorporation into its programme for standard pyrheliometry.

Pyrheliometer no. 146 was returned to Sweden for rebuilding, and was received in December 1950. Prior to its return to Pretoria it had been compared (in the Swedish Meteorological Service) against Michelson actinometer no. 535 which, in turn, had been standardized by Ångström pyrheliometers nos. 153 and 158 at Stockholm. These comparisons were made on 30 September and 5 October 1950. The Swedish scale is Å + 3·5 per cent, not the Smithsonian reference, so conversion to the original Ångström scale only involves arithmetic. The tube in which the rebuilt no. 146 was mounted for standardization was stated to be exactly similar to the one retained at Pretoria as a standard mounting. This rebuilt pyrheliometer will be denoted B in the discussion which follows.

On examination, in December 1949, pyrheliometer no. 80 revealed some variation in the right-hand strip, which was traced to a crack having developed at one of the anchorages and to a loosely secured thermo-element. Repairs were effected by increasing the length of the strip by soldering 3 mm of similar manganin strip (from no. 18494 which was in poor condition) on to the broken end, thus enabling the strip to be firmly anchored, and renewing the attachment of the defective junction. When tested subsequently, no. 80 showed reasonable balance between the component strips and gave consistent results. The strips were reblackened by dry-sooting from the flame from pure camphor burning in mineral turpentine. Care was taken to obtain a very fine and even particle deposit (it should be noted that this is only possible if the surface to be blackened is held at least $\frac{1}{2}$ to 1 m above the flame and the operation carried out very slowly over a period of about an hour : it is also necessary to blow off any surplus larger particles periodically during the deposition).

In February 1950 pyrheliometer no. 80 was standardized in the Weather Bureau, by the method employed by K. Ångström (1899). The Ångström pyrheliometer is practically always operated as a secondary relative instrument. When it is used absolutely the constant (k), which enables the square of the compensation current to be translated directly into radiation incident normally to the exposed strips, is derived from

$$k = \frac{60r}{ba},$$

where r = the resistance of unit length of strip (ohm/cm),

b = the mean width of strip (cm),

a = the absorption coefficient of the blackened surface of the strip.

For this purpose a was taken as 0·982, following Guild's (1937) determination of coefficients of 0·9819 for the visible and 0·9813 for the near infra-red, for turpentine soot. In all conversions of units the value 4·185 Joules per 15° calorie was used for Joule's equivalent.

The measurements of electrical resistance were obtained, independently, in two ways with the aid of instruments standardized at the National Physical Laboratory at Pretoria and reliable to about one part in one thousand. Care was taken to allow for the difference in potential existing between the terminal of the pyrheliometer and the end of the strip, which had been anchored by soldering to the relatively heavy brass (tinned) mount by heat application, from above, through the strip. The determination of mean

resistance is naturally much easier to accomplish if a suitable length of the manganin strip to be used is available before being cut to length and assembled in position, but the existing strips had to be utilized in this instance. After re-determination of the instrument constant, pyrheliometer no. 80 was denoted A in the subsequent series of comparisons.

In November 1951 pyrheliometer B was dismantled and reassembled to remove the slight upward curvature in the strips, which had been noticed on the arrival of the radiometer from Sweden, and also to tighten up the anchorages of the strips which were becoming loose. The blackening was removed, the constant re-determined and the strips again blackened as described above. The pyrheliometer was then brought into service as B^1.

Between September and November 1952 pyrheliometers nos. B^1, A and 18494 (never in operation) were completely rebuilt as WB I, WB II and WB III, respectively. Some difficulty was experienced in obtaining a supply of manganin strip of the requisite dimensions. For WB I 3 mm wide strips of German (Isabellen-Hütte, Dillenburg, Hessen) origin were available; for WB II and WB III the more orthodox 2 mm strip had been received from a source in the United States (Baker and Co., Newark, N.J.). The material was first aged by heating for about 12 hr at 140°C. The construction procedure was as described, the copper-constantan thermocouples being attached to the under surfaces of the strips with zinc oxide paint. The strips were stretched tightly between the anchorage points before the final soldering, in order to obviate any appreciable curvature developing, and the couples made to press lightly against the electrically-insulated strips to ensure good thermal contact. Owing mainly to the thickness of the strips (thinner ones could not be obtained at the time), rather heavy heating currents are required to operate these pyrheliometers, e.g. for exposure to a radiation intensity of 1 cal cm^{-2} min^{-1} WB I has to be supplied with about 600 ma and WB II and WB III with nearly 450 ma. From the experiments carried out it was demonstrated that the variation of resistance over the normal working range of the pyrheliometers would not exceed $\pm$ 0·1 per cent of the mean value.

This group of pyrheliometers was first standardized, independently, and then intercompared. The comparisons were extended to include three Michelson-Büttner actinometers (nos. 6461, 6462 and 6465) and a Linke-Feussner actinometer (no. 21), all of which had been repeatedly compared against pyrheliometers A, B and B^1. In the case of WB I and WB III the agreement between the computed and the adopted value of the instrument constant was within 0·25 per cent; with WB II the divergence, 0·5 per cent, was slightly greater.

It should be added that the second mounting tube, originally intended for no. 18494, was modified to be as nearly as possible identical to the standard one. Tests undertaken prior to this alteration revealed differences of one or two per cent in the measured intensity, depending upon which tube was used. The difference is principally due to the arrangement of diaphragms (particularly the innermost one) contained in the tube for limiting the extent of sky ' seen ' by the exposed strips. A. Ångström (1951) has referred to this as the ' edge effect ' and says that it ' is caused by the fact that one of the strips which constitute the sensitive part of the instrument is heated electrically throughout its entire length, while the other strip is illuminated by solar radiation only to about $\frac{9}{10}$ of its length on account of the screening of the diaphragm inside the instrument. The error which thus arises is of the order of two per cent. It is, however, to some extent dependent on the convection of the air over the heated strips.' Clearly, the geometry of the tube is a matter of considerable importance in the investigation of small differences between pyrheliometers and would appear to have been largely ignored in the past. Bossy and Pastiels (1948) have attempted to compute the magnitude of divergences in observations arising

from differences in instrumental aperture and from the angular distribution of the circumsolar sky radiation. The Ångström pyrheliometers when mounted in the Pretoria tubes have a rectangular aperture with plane angles, on the average, of 25 and 6 degrees. The fraction of the exposed sky area is $\frac{25\times 6\,\pi}{180^2\times 2} = 0{\cdot}0073$ hemisphere. This may be compared with the circular aperture of Abbot actinometer s.I 43 with tube 32 cm long. In this case, each point on the upper surface of the silver disc is exposed to a cone of sky 6 degrees in diameter, or a fraction of hemisphere of 0·0013. For specimens of the latter type of radiometer constructed prior to 1926, the sky area is increased to 0·0043 hemisphere by virtue of the employment of a shorter tube (15 cm). It will be noticed that if the effect of the aperture 'wings' of this (the original) model of the Ångström pyrheliometer could be ignored, then the effective area of sky observed is very similar to that with the modern (e.g. Pretoria) version of the silver-disc actinometer.

The constants of the six Weather Bureau Ångström pyrheliometers and the Abbot actinometer used in the comparisons at Pretoria are given in Table I. It may be remarked that the value found for A, after the repairs, practically agrees with the original Uppsala determination of 1906 which gave a constant of 16.00.

TABLE 1. PYRHELIOMETER CONSTANTS

Pyrheliometer	Certified	Date	Constant	Pyrheliometric scale
A	Pretoria	Feb. 1950	15.96	Original Ångström
B	Stockholm	Sept.-Oct. 1950	14.10	Original Ångström
B^1	Pretoria	Nov. 1951	13.37	Original Ångström
WB I	Pretoria	Oct. 1952-Apr. 1953	2.658	Original Ångström
WB II	Pretoria	Oct. 1952-Apr. 1953	5.282	Original Ångström
WB III	Pretoria	Oct.-Dec. 1952	5.333	Original Ångström
S.I 43	Washington	Jan. 1951	0.3889	Smithsonian 1913

3. THE COMPARISONS AT PRETORIA

These comparisons may conveniently be classified as follows:

(a) Ångströms A and B; 3-14 March 1951 (original Uppsala tube — U and unmodified Cambridge tube — C)

(b) Ångströms A and B; 26 September-19 October 1951 (Uppsala tube and Cambridge tube after first modification — Cm^1)

(c) Ångströms A and B; 5-7 November 1951 (Uppsala tube and Cambridge tube after final modification — Cm^2)

(d) Ångströms A and B^1; 14-17 November 1951 (as in (c))

(e) Ångström WB I and Michelson 6465; 28 October 1952-16 April 1953 (Uppsala tube)

(f) Ångströms WB I and WB II; 15 November 1952-16 April 1953 (as in (c))

(g) Ångströms WB I and WB III; 28 November-27 December 1952 (as in (c)).

A special switching arrangement was provided to enable one or other of the Ångström pyrheliometers under comparison to be rapidly connected with the measuring equipment. For observations made during the summer months at times when the sun was near the zenith, the instrument leads were extended so that the pyrheliometers could be

placed well away from the walls of buildings. There were thus no occasions with doubt as to the relative areas of sky taken in by the pyrheliometers. During all comparisons the sky was cloudless or very nearly so.

The usual observing procedure for intercomparison of two compensation pyrheliome ers using a set of measuring equipment common to both (viz. A_R B_R B_L A_L etc.) was adopted. On the majority of occasions, one pyrheliometer or the other was operated absolutely, i.e. the instrument constant was checked during the observation. The value of k rarely departed by as much as 0·2 per cent from the adopted figure.

A summary of these seven sets of comparisons is presented in Table 2. The 60 comparisons were obtained on a total of 33 days.

TABLE 2. Pyrheliometric comparisons at Pretoria; 3 March 1951-16 April 1953

Set	No. of Comparisons	No. 1 Pyr.	No. 1 Tube	No. 2 Pyr.	No. 2 Tube	I (cal cm^{-2} min^{-1}) No. 1	I (cal cm^{-2} min^{-1}) No. 2	No. 1/No. 2
(a)	2	A	U	B	C	1·395	1·425	0·979 } 1·002
	2	A	C	B	U	1·390	1·354	1·026 }
(b)	3	A	U	B	Cm^1	1·373	1·389	0·988 } 1·000
	5	A	Cm^1	B	U	1·280	1·265	1·012 }
(c)	3	A	U	B	Cm^2	1·412	1·415	0·998 } 0·999
	3	A	Cm^2	B	U	1·408	1·409	0·999 }
(d)	12	A	Cm^2	B^1	U	1·378	1·377	1·001 ± 0·003
(e)	13	WB I	U	M6465		1·383	1·382	1·001 ± 0·002
(f)	9	WB I	U	WB II	Cm^2	1·325	1·326	0·999 ± 0·004
(g)	8	WB I	U	WB III	Cm^2	1·438	1·438	1·000 ± 0·004

Michelson actinometer no. 6465 was standardized directly against Ångström pyrheliometers A, B and B^1 during the period 12 July 1951-18 June 1952. The mean sensitivity (derived from 22 comparisons) worked out at 0·02095 cal cm^{-2} min^{-1} per scale division at 25°C. A temperature coefficient of − 0·2 per cent per deg. C was established: this was applied in the (e) set of comparisons summarized above. The mean deviation of the original comparisons upon which the Michelson factor depended was ± 0·35 per cent.

The series of comparisons involving the other substandards referred to in the introductory section will not be discussed here. It is sufficient to mention that they served to confirm the continuity of the A, B and B^1 pyrheliometers with the later WB group. However, these comparisons revealed the useful fact that a single observation derived from a series of ten readings made with the Michelson-Büttner type of radiometer (as manufactured by Günther and Tegetmeyer, Brunswick) is, on the average, accurate to within one-half per cent, provided proper control by absolute pyrheliometers is regularly applied and that precaution is taken to avoid disturbances due to wind and vibration.

4. Comparisons between the South African and Belgian Congo standards

During a visit of Dr. W. Schüepp of the Belgian Congo Meteorological Service to Pretoria the opportunity was taken to compare one of the Belgian Congo Ångström-type pyrheliometers with the Pretoria group of standards. This pyrheliometer (no. S.I 11) is a Smithsonian-modified version (1942) of the European instrument. It was standardized at Washington early in 1952. Unfortunately, unsuitable weather conditions restricted the comparisons to four in number, so in August 1954 the present writer took Ångström pyrheliometer WB II and Linke-Feussner actinometer no. 21 to Leopoldville to continue

comparisons. Three series were obtained, the results being completely consistent with those of two years before. The Congo radiometers were Ångström no. 119 and Linke-Feussner actinometer no. 79. The former is of orthodox construction with a tube similar to those of the Pretoria pyrheliometers : it was standardized at Davos in 1951 against Ångström pyrheliometer no. 210 which is referred to the Å + 3·5 per cent scale. The two Linke-Feussner actinometers are thermopile instruments calibrated locally. No. 21 was checked at Pretoria immediately before and after the Leopoldville comparisons (the temperature dependence of this radiometer has been established, at Pretoria, and due account is taken of this factor in reducing the observations). The tube of the Pretoria Linke is such that the half-aperture angle is 6 degrees. The Leopoldville Linke is very similar.

The comparisons obtained on both occasions are detailed in Table 3. It should be remembered that with the first set the ratio BC/SA gives the relationship between radiometers scaled according to the Washington Smithsonian scale and the Pretoria Ångström scale; with the second set the relationship is of the Davos ' pseudo-Smithsonian ' scale to the Pretoria one.

The outputs of the Linke-Feussner actinometers were detected by a precision potentiometer. Ångströms WB II and 119 were operated quite independently of one another.

TABLE 3. Comparisons with Belgian Congo pyrheliometers

		LAT	South African pyrheliometers	Belgian Congo pyrheliometers	I (cal cm^{-2} min^{-1}) SA	BC	BC/SA	
Pretoria 1952	Nov. 27	12·41–13·21	WB I, WB III	S.I.11	1·432	1·467	1·024	1·035
	28	11·05–11·33	WB I, WB III	S.I.11	1·400	1·461	1·043	
	Dec. 2	10·26–10·41	WB I	S.I.11	1·400	1·456	1·040	
	4	10·00–10·25	WB I	S.I.11	1·427	1·475	1·034	
Leopoldville 1954	Aug. 26	14·14–14·40	L. 21	A. 119, L. 79	0·738	$0·765_5$	1·037	1·034
	26	14·50–15·02	WB II, L. 21	A. 119, L. 79	$0·722_5$	0·744	1·030	
	30	11·02–11·18	L. 21	A. 119, L. 79	1·035	$1·071_5$	1·035	
				Mean	1·165	1·206	1·035 ± 0·004	

5. The Ångström versus the Smithsonian scale

(a) *The sun as the source of radiant energy*

For nearly half a century there has been considerable controversy in respect of the true relationship between the two scales of pyrheliometry. The differences found by various investigators in many countries have ranged from about 2·5 to 6 per cent, the mean acceptable value being put, by the World Meteorological Organization (1953b), at 3·5 per cent. If we correct for the known error of the 1913 Smithsonian scale (Hoover and Froiland 1953), the range becomes 0-3·5 per cent with 1.0 per cent as the general value of the difference.

It was considered that the favourable skies for pyrheliometric work (which are so frequently a feature of the climate of Pretoria sited at an altitude of nearly 1400 m in the belt of the southern semi-permanent anticyclone) might assist in the assembling of highly reliable comparative data in respect of the relationship between the scales. A further advantage was, of course, the availability of a group of Ångström pyrheliometers the constants of which could be verified independently. The Abbot silver-disc actinometer (s.I 43) is a modern one standardized, in the Smithsonian Institution, only a few months prior to the commencement of this part of the programme. Therefore there is every

TABLE 4. ABBOT-ÅNGSTRÖM COMPARISONS AT PRETORIA: 16 NOVEMBER 1951–28 NOVEMBER 1952

Year	Month	Day	LAT	Abbot Temp. (°C)	Cycles	Ångström Pyr. etc.	Solar Altitude (degrees)	Optical Air Mass	Synoptic Air Mass	Moisture Content (cm)	I (cal cm^{-2} min^{-1}) Abbot	Ångström	Abbot Ångström
1951	Nov.	16	11·57-12·22	30	3	A, B1	82·5	0·86	mT	1·85	1·441	1·390	1·037
		17	11·45-12·03	27	3	A, B1	83·0	0·86	mT	1·97	1·427	1·393	1·025
		20	12·05-12·23	29	3	B1	82·9	0·86	mTm	2·27	1·376	1·330	1·035
		20	12·30-12·48	30	3	B1	79·1	0·87	mTm	2·27	1·367	1·317	1·038
		21	12·27-12·53	31	4	B1	79·0	0·87	mTm	2·82	1·386	1·353	1·025
		23	12·02-12·56	25	9	B1	81·4	0·86	mTm	1·97	1·433	1·375	1·042
		26	10·31-10·55	28	4	M	71·7	0·90	cT	1·70	1·498	1·456	1·029
		28	09·24-09·54	22	5	B1	57·5	1·01	S	1·14	1·345	1·300	1·035
		28	11·31-12·01	25	5	B1	84·5	0·85	S	1·14	1·390	1·347	1·032
	Dec.	6	12·08-12·38	30	5	B1	83·8	0·86	mTm	2·43	1·475	1·420	1·038
		12	11·03-11·33	27	5	L	80·1	0·87	mTm	2·83	1·486	1·449	1·026
		12	11·55-12·24	29	5	B1	86·6	0·85	mTm	2·83	1·508	1·448	1·042
		19	10·51-11·51	28	10	B1	75·2	0·89	mTm	2·89	1·448	1·386	1·045
		21	08·43-09·13	24	5	B1	48·7	1·13	mTm	3·03	1·333	1·292	1·032
		21	10·35-10·59	27	5	B1	73·3	0·90	mTm	3·03	1·426	1·370	1·041
1952	Feb.	21	11·36-12·01	29	3	B1	74·8	0·89	mTm	2·92	1·378	1·325	1·040
	Mar.	3	14·19-14·44	24	3	M	49·4	1·12	cT	1·57	1·413	1·373	1·029
	Apr.	9	11·17-11·58	22	5	B1	56·5	1·02	mTm	2·24	1·426	1·377	1·036
	Nov.	18	12·15-12·39	27	4	WB I, WB II	80·8	0·86	cT	1·65	1·536	1·483	1·036
		28	11·05-11·33	28	3	WB I	79·6	0·87	mTm	4·03	1·446	1·402	1·032
Mean or total				28	92			0·91		2·33*	1·427	1·379	1·035 ± 0·005

* Over the world as a whole the air contains, on the average, the equivalent of about 2·5 cm of water.

reason to believe that the results of the observations taken with the radiometers are faithful reproductions of the basic pyrheliometric scales. Michelson actinometer no. 6465 and Linke-Feussner actinometer no. 21 were also employed in the series of comparisons.

The comparisons are set out in detail in Table 4. The period extends over 12 months; nearly 100 individual sets of comparisons are included.

The values listed in column 5 refer to the air temperature in the vicinity of the Abbot actinometer mounted on the roof of the main building of the Weather Bureau : the temperatures of the Abbot instrument were several degrees higher. Column 8 contains the solar height for the mean of the observing interval and column 9 the corresponding value of the optical air mass corrected for the altitude of the observing station.

In column 10 the synoptic air mass type prevailing at the time of comparison is entered. The analysed surface maps available for 0800 and 1400 LST were consulted, together with the relevant upper-air diagrams of the morning radiosonde and aircraft ascents over South Africa. The classification used here is as follows :

mT	maritime tropical,
mTm	maritime tropical modified (continentally),
cT	continental tropical,
S	subsiding air.

It will be noticed that air originating within the tropics was present on practically every occasion.

The total moisture content of the vertical column over Pretoria, expressed in cm of precipitable water, is given in column 11, computed from the local radio-soundings up to the 300-mb level. Moisture content varies by a factor of nearly four. The smaller values are associated either with subsidence or with relatively dry tropical air masses. It is of interest to compare the ratio Abbot/Ångström for all occasions (viz. $1{\cdot}035 \pm {\cdot}005$) with that for moisture contents of less than 2 cm precipitable water, of which there are eight instances. The latter value works out at $1{\cdot}033 \pm {\cdot}004$, which is practically identical to the overall mean. Clearly, any scattering effects correlated with water vapour do not exert significant influence upon such pyrheliometric comparisons.

An attempt was also made to try to assess the extent of the effects of atmospheric turbidity (which results chiefly from the concentration of particulate matter such as dust and smoke in the air) on the comparisons. On 21 February, 3 March and 9 April 1952, measurements at, or close to, the time of the comparisons were made with Michelson 6465 which is fitted with Schott standard filters OG1 and RG2, the transmission characteristics of which were subsequently checked at the National Physical Laboratory in Pretoria. Although Schüepp (1949, 1952) has extended the well-known work of Ångström (1929, 1930) it was decided to employ Ångström's method as it stands in order that the results could be comparable with others derived similarly elsewhere. Ångström found that the intensity of solar radiation at the earth's surface (I) may be represented by

$$I = \int_0^\infty I_\lambda' \, q_\lambda{}^m \, e^{-\frac{\beta m}{\lambda^\alpha}} \, d\lambda - F,$$

where m = the optical air mass,

I_λ' = the intensity at wavelength λ outside the earth's atmosphere at earth's distance,

q_λ = the Rayleigh coefficient of molecular scattering at wavelength λ,

β = the turbidity coefficient, varying with the amount of scattering by dust and other particles,

α = the contrast exponent, depending upon the mean effective size of scattering particles in the atmosphere, and

F = the selective absorption by water vapour.

The water-vapour absorption, F, is almost entirely in the region of wavelength greater than 700 mμ. Therefore, if we subtract the transmission-corrected filter measurement from the whole-spectrum one, F is eliminated. Denoting the portion of the total intensity for all wavelengths up to 525 mμ by I_1, and by I_0 that passed by the OG1 filter (corrected),

$$I_1 = I - I_0 = \int_0^{0{\cdot}525\mu} I_\lambda' \, q_\lambda{}^m \, e^{-\frac{\beta m}{\lambda^\alpha}} \, d\lambda.$$

A similar expression holds for the red filter.

The values of β determined, in this way, for the 6 sets of observations, at Pretoria, are reproduced in Table 5. The turbidity coefficients derived from the OG1 filter are denoted β_0 and those from the RG2 filter, β_R.

TABLE 5. Turbidity coefficients at Pretoria; February-April 1952

Month	Day	LAT	I cal cm^{-2} min^{-1}	Optical air mass	β_0	β_R
Feb.	21	10·10–10·31	1·289	0·96	0·125	0·125
		11·15–11·28	1·319	0·89	0·115	0·115
		12·13–12·29	1·281	0·87	0·135	0·140
Mar.	3	14·45–14·58	1·321	1·20	0·100	0·090
Apr.	9	10·51–11·13	1·369	1·05	0·065	0·070
		12·06–12·20	1·372	1·01	0·070	0·070

The two sets agree closely, the mean value of β for the three days being 0·100. From Table 4 it will be seen that, in each instance, the synoptic air mass was of tropical type. These three days were quite typical of the whole series of comparisons as the mean Abbot/Ångström ratio is 1·035.

In comparison with β for Pretoria under these conditions, Schulman's (1943) tabulations of β, determined with filters similar to those described above, for the years 1934-1939 at Blue Hill Observatory, near the Massachusetts coast, were consulted. For his four classes of tropical maritime air mass, the average value of β is 0·085 – a figure which agrees well with the Pretoria one. At Kew (Stagg 1950) on days of ' ceiling radiation intensity,' the coefficient is generally between 0·05 and 0·07. Around midday in spring values there may exceed 0·2, even on days of ' high total radiation.' In contrast to the smoke-polluted air of London, the clear arctic air of Spitzbergen during summer months is characterized by values of β' (Schüepp 1949) which are frequently below 0·02 and sometimes below 0·01 (Olsson and Schüepp 1950). At the high-level station at Davos, in the Swiss Alps, β varies between 0·02 and 0·03 on the average (Ångström 1930).

Hence, it is a plausible argument that the general atmospheric conditions over the high plateau, at Pretoria, throughout the period of comparisons of the two types of sub-standard pyrheliometer, were typical of tropical maritime air, and that the turbidity was no less marked than on the clearer occasions at lower-level localities in the temperate regions. The observing conditions may be termed good but were by no means outstanding: for example, the average atmospheric transmission of the cloud-free path over the period, as expressed by I/I', varied between 0·63 and 0·73 (mean 0·68). For Brukkaros, a remote mountain peak of approximately 1700 m altitude in South-West Africa, the transmission coefficient for unit air mass, for the clearest skies, has been shown to be about 0.80 (Drummond and Wentzel 1955).

On several occasions (not listed in Table 4) the divergence between the pyrheliometers reached values of between 5 and 6 per cent (as has been reported by other investigators), but scrutiny of the trends of the individual instrumental readings of the two series revealed that, in these cases, the Abbot actinometer was yielding spurious results which were in no way connected either with the incoming solar radiation or with the state of the sky. It was found that instrumental vagaries of this nature were associated with high ambient temperature and still-air conditions. The matter was not investigated further, but it would seem that the silver-disc instrument can be influenced by some effect caused by over-heating.

In a recent discussion of similar comparisons undertaken at Davos, Courvoisier (1954) presents results which certainly do not lend material support for the contention that the wide range of Abbot-Ångström differences, found elsewhere, is mainly dependent upon the differing conditions of atmospheric turbidity. It is therefore the view of the present writer that such effects are much less pronounced than hitherto has been believed and, *under tolerable conditions of observing*, should not lead to departures from the ratio of 1·035 of more than about one-half per cent on the average. From comparisons made in the turbid atmosphere of London, Watson (1923) found precisely this value for the relationship between the pyrheliometric scales.

(*b*) *An artificial source of radiant energy*

The Ångström and Smithsonian scales have been compared in the laboratory on two previous occasions (Guild 1937; Eldridge 1952). These comparisons, however, were not entirely independent of one another as the same silver-disc instrument was employed. Also, the later set included an Ångström pyrheliometer (no. 100A) which was used in the earlier set. The silver-disc actinometer (s.I 28) is a copy of the old model with the short tube (exposed sky area of ·0043 hemisphere) and was originally standardized, at Washington, in 1916. It would appear that the value of the instrument constant given in the certificate has never been verified since.

It was decided to include such a comparision in the present study. The source used was a gas-filled, tungsten-filament 2000 w projector lamp mounted in a forced-ventilated housing. A thermally insulated diaphragm system is bolted to the front of the lamphouse. The two innermost screening diaphragms are of polished aluminium and the outermost is of hardboard painted dull black.

The radiation source is controlled by an open-scale manually operated rheostat, the applied A.C. mains voltage (235 v) being first regulated by means of an automatic transformer. A sub-standard voltmeter can be readily switched across the terminals of the lamp. The precision of this meter was verified at the South African National Physical Laboratory. A voltage variation of 0·25 v in the lamp circuit can be detected; this corresponds to a change in emitted radiation flux of 0·1 per cent. Normally, the voltage is reduced to 160 v before the lamp is switched in. The voltage is then gradually raised, over a two-minute interval, to the working value. As the lamp is rated at 250 v it is always slightly under-run. It is of interest to mention that in the first 115 hours of operation the overall change in the lamp emission, as measured at 10 points between 60 and 160 cm from the source, did not exceed 0·1 per cent, which is the absolute accuracy of the group of Ångström pyrheliometers employed for detection. After five years of useful service this lamp was recently replaced by a similar one; the output was found to have gone down by only four per cent but the colour temperature (originally estimated at 3060°K, by the makers, for 235 v) had decreased markedly, especially during the last year or so of its life.

The radiation bench was standardized, in December 1951, Ångström pyrheliometers A and B[1] being used : 25 measurements were made at distances of from 62 to 166 cm from the source; the resulting measurements fitted the inverse square law very closely. The same measuring equipment was used with the pyrheliometers as in normal operation in sunlight. A second standardization was carried out with the Linke-Feussner actinometer (which has a non-selective receiver) in order to establish the value of the correction required to compensate for the lower response of the soot-blackened pyrheliometers in the beam of the lamp as compared with the sun whose energy is, of course, of markedly different spectral composition. This correction (+ 2·8 per cent) was not applied in the Abbot/Ångström comparisons as it was considered that the selectivity of the blackened silver-disc (understood to be a fine grade of lamp-black well mixed with alcohol and a small quantity of shellac) ought to be very similar to that of the Ångström strips. There was no way of verifying this assumption, which has also been accepted by Guild and Eldridge.

The method of operation of the silver-disc instrument, both in the laboratory and naturally, was exactly that laid down by the Smithsonian Institution (Aldrich 1949) upon which the Washington standardization of the actinometer is based. In view, however, of the smaller temperature differences to be observed, a higher-power lens was fitted than that used with the outdoor comparisons.

The artificial source was never operated continuously for periods exceeding 45 min at a time to avoid unnecessary fatigue. Over runs of 15-20 min it was demonstrated that, when the mains voltage was reasonably steady, constancy of emitted flux could easily be maintained to better than two parts in one thousand. Such ' quiet ' spells were usually selected for the Abbot measurements. On the 4 days concerned repeated checks on the basic bench standardization were made with one or other of the pyrheliometers, which were always used absolutely.

The results of the series of comparisons are set out in Table 6. It will be noticed that hese comparisons fall within the period of those obtained under natural conditions of exposure.

TABLE 6. Abbot-Ångström comparisons in the laboratory ; December 1951-April 1952

Month	Day	Distance from source (cm)	Abbot s.I 43 Temp. (°C)	Cycles	I (cal cm^{-2} min^{-1}) Abbot	Ångström	Abbot / Ångström
Dec.	11	108·0	26	3	0·2020	0·1975	1·023
		128·0	27	3	0·1450	0·1410	1·028
		97·8	28	3	0·2055	0·1980	1·038
Apr.	16	117·1	24	5	0·1740	0·1685	1·032
	18	122·1	23	8	0·1585	0·1545	1·026
		102·1	24	12	0·2265	0·2195	1·032
	19	127·1	24	7	0·1290	0·1255	1·028
		127·1	25	10	0·1290	0·1255	1·028
Mean or Total			25	51	0·1712	0·1663	1·029 ± 0·003

The relationship between the scales as demonstrated by the particular radiometers used is 1·029. This compares with the value of 1·027, found by Guild and verified by Eldridge, and is 0·6 per cent lower than that established in Table 4. The scatter of the individual results about the mean is quite typical of the personal and instrumental errors

involved in the intercomparison of a pair of pyrheliometers of the types used here. That the results are independent of the position on the radiation bench is proved by comparing the mean for positions of 117-128 cm from the source with the mean for the whole series. The former works out at $1{\cdot}028_5$.

6. Conclusions

These may be summarized as follows :

(a) The true relationship between the Ångström (uncorrected) and the Smithsonian (1913) scales, for average conditions of natural exposure of pyrheliometers in which the respective scales are embodied, is Smithsonian/Ångström = 1·035.

(b) The divergence between the results of measurements made with the Ångström (original type) pyrheliometer and the Abbot silver-disc actinometer due to conditions of atmospheric turbidity is much less than has hitherto been proposed, and it is likely that the range in such divergence will not exceed one per cent except, perhaps, under exceptionally poor conditions of comparison.

(c) The fundamental pyrheliometric standards of reference maintained at Davos, Leopoldville, Pretoria, Stockholm and Washington are in close accord.

(d) The results of the study would appear to indicate that employment of the original Ångstrom scale yields values which are too low by between $1{\cdot}0 \pm 0{\cdot}5$ and $0{\cdot}6 \pm 0{\cdot}3$ per cent, depending upon whether the pyrheliometer is operated under conditions of average clear skies or with freedom from effects of atmospheric scattering. In the latter instance Guild's findings were confirmed.

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A determination of the relation between wind and sea-surface drift

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Summary

The drift of plastic envelopes floating close to the sea surface is related to the gradient wind. The results indicate that in westerly situations a shallow layer near the sea surface moves in approximately the same direction as the gradient wind with about 2 per cent of its velocity.

1. Introduction

In connexion with oil pollution investigations being made by the Ministry of Transport, the National Institute of Oceanography has studied the drift of cards enclosed in plastic envelopes, floating under the influence of sea and wind in the Atlantic Ocean west of the British Isles (Lawford 1956). The cards were released by aircraft of Coastal Command over a wide arc from the Bay of Biscay to the North of Scotland, a bundle of ten being dropped every ten nautical miles. The first drop in February 1954 failed owing to the use of a too thin plastic cover, but the last three were more successful with over 30 per cent returned from the second and third drops in May and September 1954. The envelopes float close to the surface, so that the results probably refer to the movement of a shallow layer of water of the order of a few centimetres thick.

This investigation considers the gradient wind over the drift path of selected envelopes and relates wind speed and direction to envelope speed and direction.

2. Observations

Drift data have been used from cards showing the shortest drift time for each 50 mi of laying path. By this selection it was hoped to obtain results representative of wind-driven surface water. The drift times ranged from 27 to 145 days, while the shortest distance between release and recovery position varied from 132 to 638 mi.

The Meteorological Office *Daily Weather Report* charts were used for the wind computations. The gradient wind, which is a reasonable estimate of the wind at 2,000 ft, was obtained from the isobar spacing, the usual corrections being made for the variation with latitude and curvature of the isobars. The area of sea concerned was divided into squares of dimensions $2\frac{1}{2}°$ lat. by 5° long., and the wind vector was then found in each square at intervals of 6 hr. 10-day means were computed for each square and the results are given in Table 1.

The second envelope drop was made on 5 May 1954 and the third on 1 Sept. 1954. From 5 May until 20 June, the winds in general had a marked northerly component, whereas after that date the wind came more from the NW, at least until the end of July which covers the period of interest for the second drop. In contrast, during the drift of the third drop envelopes there existed persistent and strong W to SW winds with no marked change in direction.

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TABLE 1. 10-DAY MEAN WIND VECTORS

Date 1954	Lat. (°N) Long. (°W)	55-57½ 15-20	55-57½ 10-15	55-57½ 5-10	52½-55 15-20	52½-55 10-15	50-52½ 15-20	50-52½ 10-15	50-52½ 5-10	47½-50 15-20	47½-50 10-15	47½-50 5-10	45-47½ 7½-12½	45-47½ 2½-7½
May	5-10	4·9/339*	5·7/025		7·2/051	9·9/048	6·8/042	8·5/031		7·1/045	8·7/037		3·1/042	1·3/108
	11-20	5·7/042	2·2/062		3·9/066	2·5/151	2·8/049	0·42/267		0·7/060	0·6/298		5·5/225	5·1/212
	21-31	2·5/022	7·5/010		6·1/064	6·0/045	6·7/083	5·5/057		4·2/065	5·1/053		5·0/062	3·4/058
June	1-10		3·0/305	3·6/308	0·84/255	2·4/312	2·1/148	3·0/363	1·9/018	5·1/130	4·6/072	4·2/062	6·4/097	8·3/049
	11-20		2·9/070	5·9/061	8·1/073	8·4/071	8·4/077	6·7/079	4·5/082	6·3/085	6·0/081	4·9/077	6·0/112	3·9/081
	21-30		12·0/114	10·0/121	10·4/128	10·4/128	9·6/126	8·4/136	4·9/138	7·3/137	7·4/146	6·7/146	5·1/176	2·1/154
July	1-10		9·0/119	6·6/099		11·1/117		9·0/120	8·7/120		9·6/123	8·2/118	6·0/126	6·0/119
	11-20		9·8/090	6·0/097		10·8/102		8·9/117	7·7/119		9·1/114	8·2/114	5·1/154	5·4/119
	21-31		12·8/113	8·9/096		12·8/110		10·8/109	10·6/105		9·0/114	8·5/110	7·0/124	5·7/124
Aug.	1-10		6·0/131	6·0/080					7·7/081			8·4/071	9·8/110	10·0/098
	11-20		2·0/315	3·6/238									6·0/123	5·3/090
	21-31													4·2/129
Sept.	1-10	5·2/081	5·4/037		8·7/089	6·6/082	9·9/093	9·3/067	5·7/058	9·2/094	8·8/079	7·2/061	7·8/076	5·4/065
	11-20	13·0/106	13·2/106		14·0/119	12·2/102	14·2/111	10·9/108	8·3/102	7·9/107	8·7/107	6·4/104	7·8/102	6·9/089
	21-30	5·2/095	7·5/101		9·4/089	8·2/088	10·2/090	8·6/100	7·1/108	8·2/083	6·7/100	5·5/099	6·9/109	6·3/096
Oct.	1-10		11·4/068			9·9/078		6·1/063	4·5/079		8·0/073	4·8/080	3·0/083	2·1/073
	11-20		6·9/055			8·1/053		10·5/057	11·1/060		11·7/049	9·3/052	7·8/045	4·6/047
	21-31		6·6/088			8·2/081		9·7/079	8·9/082		8·5/084	8·2/085	8·9/085	7·5/076
Nov.	1-10		11·2/113						2·7/162		6·9/207	3·0/198		
	11-20		11·6/079						5·6/083		6·3/063	4·1/074		

* The first number for each square is the wind speed (m/sec); the second is the direction (degrees) *towards* which the wind is blowing.

3. Method of analysis

In any drift experiment we know the date and position of release and the date and position of recovery. From these we obtain **S**, the shortest possible path, and T the longest possible drift time.

It is assumed firstly that the surface-water speed, u_s, is proportional to the gradient-wind speed, W, and secondly that the directions of gradient wind and water movement are inclined at a constant angle, θ, independent of speed. The first relation may be expressed as

$$u_s = kW \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where k is known as the 'gradient-wind factor.' For convenience W is in m/sec and u_s in cm/sec so that k appears as a percentage.

The drift may be divided into n periods of time, during each of which the wind affecting the surface water in the locality of the particular envelope is constant in speed and direction. The resultant displacements are $\mathbf{d}_1$, $\mathbf{d}_2$, ... n terms. Thus

$$\mathbf{d}_1 + \mathbf{d}_2 + \ldots . n \text{ terms} = \mathbf{S} \quad . \quad . \quad . \quad (2)$$

and

$$T_1 + T_2 + \ldots T_i + \ldots n \text{ terms} = T \quad . \quad . \quad (3)$$

T_i being the period of time during which the wind acts on the sea to produce the i^{th} component of drift.

Resolving displacements along and perpendicular to **S**,

$$W_1T_1 \cos(\alpha - \theta) + W_2T_2 \cos(\beta - \theta) + \ldots + n \text{ terms} = S/k \quad . \quad (4)$$

$$W_1T_1 \sin(\alpha - \theta) + W_2T_2 \sin(\beta - \theta) + \ldots + n \text{ terms} = 0 \quad . \quad (5)$$

These last two equations are independent and may be used to find the two unknowns k and θ uniquely, α, β, etc., being defined by equations of the type $\cos\alpha = \mathbf{W}_1 \cdot \mathbf{S}/W_1S$.

A graphical method was used to solve Eqs. (4) and (5). It consisted of plotting the air displacements (wind vector against time) over the duration and approximate path of the envelope in question. θ is then the angle between the resultant air displacement and the shortest path vector between release and recovery positions, and k is the ratio of the displacements.

As an example, consider the drift of card No. 7234 : Release position : lat. 52°12′N, long. 19°35′W, date 1.9.54. Recovery position : lat. 52°10′N, long. 10°28′W, date 28.9.54. $\mathbf{S}$ = 342 naut. mi towards 91° E of N. T = 27 days.

The squares containing the shortest displacement vector are 50 to 52½°N, 15 to 20°W and 50 to 52½°N, 10 to 15°W. Plotting wind vectors successively, we obtain the wind travel during the drift time : 27·9 × 466 naut. mi towards 86°, whence, using Eqs. (4) and (5)

$$k = 342/(27{\cdot}9 \times 466) \times 100 = 2{\cdot}6$$

and

$$\theta = 91° - 97° = -6°$$

θ being positive when the water moves to the right of the wind.

4. Results

Values of k and θ obtained for the selected envelopes are given in Tables 2 and 3, and are also shown in Figs. 1 and 2. Mean values, together with the standard deviations

TABLE 2. RESULTS FOR SELECTED ENVELOPES FROM SECOND DROP

Card No.	Date dropped	Release position Lat.N.	Long.W	Recovery position Lat.N	Long.W	T (days)	S (naut. mi)		k	θ
3830	5 May	48°59′	16°34′	47°30′	3°10′	99	560	100°	2·14	2
3885	5 May	49°56′	16°42′	47°57′	4°30′	96	520	103°	2·06	4
3930	5 May	50°31′	16°50′	48°30′	4°45′	97	502	106°	1·86	7
3990	5 May	51°33′	16°57′	48°28′	4°45′	85	512	110°	2·20	8
4045	5 May	57°32′	17°01′	52°15′	10°10′	60	260	94°	1·94	14
4084	5 May	53°11′	17°06′	54°10′	10°00′	56	258	76°	1·94	− 2
4128	5 May	53°43′	17°12′	55°00′	8°20′	53	323	77°	2·66	4
4163	5 May	54°24′	17°18′	55°15′	7°30′	58	355	80°	2·80	− 1
4229	5 May	55°13′	17°24′	55°10′	7°30′	58	320	92°	2·26	9
4245	5 May	55°32′	17°24′	54°15′	8°45′	62	312	101°	2·00	15
4458*	5 May	46°00′	55°0′	43°35′	5°56′	58	150	170°	2·14	65
4503*	5 May	46°00′	71°0′	43°40′	6°00′	64	158	160°	1·80	52
4537*	5 May	46°00′	74°5′	43°40′	5°30′	64	173	144°	2·00	36
4595*	5 May	46°11′	83°5′	43°20′	8°20′	63	175	180°	2·06	72
4617*	5 May	46°28′	83°8′	43°45′	7°50′	61	166	168°	2·14	61
4665	5 May	47°22′	8°37′	46°35′	1°50′	74	302	99°	3·14	− 13
4716	5 May	48°09′	8°38′	48°24′	4°41′	52	160	85°	2·20	− 12
4774	7 May	48°14′	8°52′	48°10′	4°30′	56	188	93°	2·46	− 14
4832	7 May	47°38′	9°52′	43°25′	4°15′	90	356	135°	2·40	28
4879*	7 May	47°14′	10°32′	43°12′	9°00′	71	250	165°	2·40	52
4946	7 May	47°18′	11°24′	43°25′	3°05′	95	435	123°	2·40	7
4951	7 May	47°27′	11°34′	43°40′	5°50′	92	350	132°	2·06	16
5015	7 May	48°21′	12°04′	44°50′	1°15′	111	518	115°	1·60	8
5039	7 May	48°39′	12°14′	43°30′	1°35′	111	568	125°	1·60	18
5049	7 May	48°48′	12°20′	48°25′	9°10′	63	132	100°	1·13	3
5085	7 May	48°32′	12°48′	48°20′	4°35′	80	344	93°	1·74	− 11
5110	7 May	48°18′	13°04′	47°48′	4°23′	97	374	97°	1·53	− 4
5255	7 May	47°32′	15°02′	44°20′	1°20′	145	638	108°	1·86	8
5300	7 May	48°04′	15°42′	46°29′	1°45′	145	614	99°	1·80	− 1
5340	7 May	48°36′	16°22′	47°20′	3°20′	94	560	98°	2·20	− 8

* These drifts which are confined to the southern part of the Bay of Biscay have large values of θ.

T is the drift time. **S** is the shortest displacement vector. The first column under **S** gives the distance between the release and recovery positions. The second column indicates the angle which the line between release and recovery positions makes with geographical North.

σ_k and σ_θ, are given in Table 4. If we regard these samples as coming from normal populations the following 95 per cent confidence intervals are obtained for the mean distributions

$$\text{2nd drop (ii)} \quad \begin{cases} 1{\cdot}92 < \bar{k}_2 < 2{\cdot}26 \\ -0{\cdot}9° < \bar{\theta}_2 < 7{\cdot}9° \end{cases}$$

$$\text{3rd drop} \quad \begin{cases} 2{\cdot}16 < \bar{k}_3 < 2{\cdot}40 \\ -5{\cdot}0 < \bar{\theta}_3 < -1{\cdot}4 \end{cases}$$

The differences may be significant, but in view of the systematic errors, which will be considered in the next section, it seems justifiable to combine the results to obtain overall mean values for $\bar{k}$ and $\bar{\theta}$; these are given in the last column of Table 4.

The surface drift is thus sensibly parallel to the isobars, i.e. in the same direction as the gradient wind. From this result and the value of 2·2 for the gradient wind factor, the surface drift in similar situations may be estimated directly from the isobaric charts.

TABLE 3. RESULTS FOR SELECTED ENVELOPES FROM THIRD DROP

Card No.	Date dropped	Release position Lat.N	Release position Long.W	Recovery position Lat.N	Recovery position Long.W	T (days)	S (naut. mi)		k	θ^* (deg.)
5394	Night of	45°56′	8°50′	46°20′	1°35′	31	315	85°	1·86	− 5
5468	31 Aug. to	45°59′	10°36′	46°46′	2°07′	51	373	83°	2·00	3
5538	1 Sept.	46°18′	11°41′	47°35′	3°10′	49	377	79°	2·14	− 1
5597		47°17′	11°21′	48°05′	4°40′	32	285	81°	2·20	− 10
5637		47°56′	11°09′	48°35′	4°50′	27	273	77°	2·60	− 8
5668		48°07′	11°29′	48°38′	4°28′	28	305	84°	2·54	− 15
5702		47°44′	12°21′	48°40′	4°35′	41	329	81°	1·86	− 4
5796		46°58′	15°00′	48°02′	4°35′	50	394	82°	2·00	3
5831		47°36′	14°22′	48°35′	4°50′	53	400	83°	1·67	5
5873		48°14′	14°46′	50°35′	2°30′	64	510	75°	2·40	− 5
5920		48°56′	15°16′	50°10′	5°30′	48	400	80°	2·46	1
5952		48°46′	15°56′	50°45′	2°40′	59	556	78°	2·66	− 1
6045		47°33′	17°21′	50°45′	2°55′	69	630	73°	3·00	− 12
6060		47°24′	17°30′	50°05′	5°32′	81	514	72°	2·14	− 12
6149		48°15′	18°27′	50°06′	5°22′	57	545	79°	2·60	− 2
6157		48°18′	18°28′	50°04′	5°17′	56	548	79°	2·60	− 2
6205		48°59′	18°59′	50°25′	5°05′	74	560	80°	2·34	− 5
6839		58°35′	19°55′	59°15′	2°55′	66	535	88°	2·26	4
6865		58°28′	20°28′	59°15′	3°02′	72	555	87°	2·00	− 1
6927		57°35′	20°14′	57°15′	7°20′	41	424	93°	2·54	5
6978		56°26′	19°58′	57°12′	7°21′	40	426	84°	2·54	− 5
7002		55°58′	19°56′	56°29′	6°59′	40	436	87°	2·60	− 2
7062		54°54′	19°46′	57°08′	7°15′	54	450	73°	2·20	− 10
7148		53°43′	19°42′	55°15′	7°35′	55	443	79°	1·94	− 6
7188		53°03′	19°39′	52°10′	10°25′	30	350	100°	2·54	2
7234		52°12′	19°35′	52°10′	10°28′	27	342	91°	2·60	− 6
7256		51°51′	19°33′	51°38′	9°50′	31	371	93°	2·60	− 3
7344		50°13′	19°24′	51°32′	9°40′	53	377	78°	1·60	− 5
7381		49°27′	19°20′	51°02′	4°20′	75	600	82°	2·46	− 2
7447		49°36′	18°39′	50°25′	5°05′	73	540	85°	2·26	2
7496		50°13′	17°45′	51°35′	4°40′	74	507	80°	1·86	− 6
7528		50°36′	17°13′	51°40′	4°10′	57	467	83°	1·94	− 1
7533		50°44′	17°02′	51°45′	4°40′	57	478	85°	2·28	–

* When the surface water movement is to the right of the gradient wind, θ is positive.

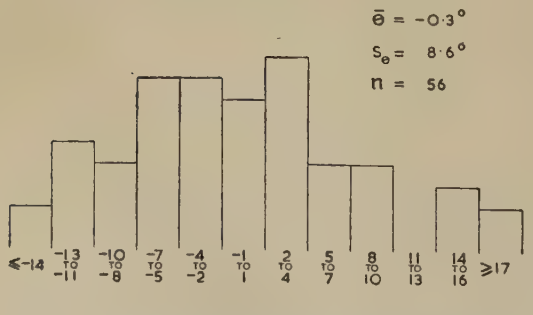

Figure 1. Angle of deviation θ° between the gradient wind and the sea surface drift (2nd and 3rd drops combined).

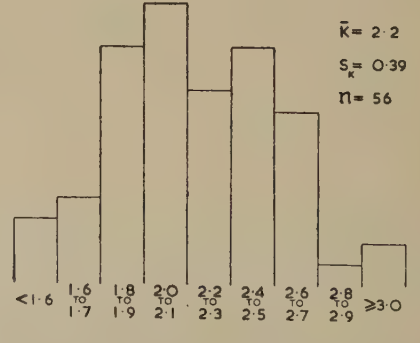

Figure 2. Distribution of the gradient wind factor.

TABLE 4. MEAN VALUES AND STANDARD DEVIATIONS OF k AND θ FROM TABLES 2 AND 3

	2nd drop		3rd drop	
	(i)	(ii)	(iii)	(ii) + (iii)
n	30	24	32	56
$\bar{k}$	2·08	2·09	2·28	2·2
σ_k	0·371	0·437	0·336	0·392
$\bar{\theta}$	14·1	3·5	− 3·2	− 0·3
σ_θ	24·1	10·7	5·1	8·6

In (ii) large-angle values in the southern Bay of Biscay have been excluded. n is the number of observations.

5. DISCUSSION

(i) *Accuracy*

The principal sources of error are :

(a) The fact that the drift of the envelope may not be a true representation of the water movement.

(b) Error in wind data.

(c) Error in drift time.

(d) Non-stationary conditions of wind and sea.

(e) Travel not due to wind, effects of permanent currents and variation of wind factor in tidal streams.

(a) In drift experiments using objects such as envelopes or bottles, there must be some uncertainty as to whether the drifting object gives a true representation of the water movement, and there may be a considerable difference in behaviour between ordinary surface bottles, surface bottles with depth drogues and ships' drift, which are influenced by layers of different thickness. The information obtained from the envelope probably applies to motion in a very thin surface layer of the order of centimetres thick, and it is to this layer that the results of this experiment are likely to apply. The envelope may also be directly affected by the wind, since in a very strong wind it might be whisked from a wave crest. This would tend to reduce the angle θ and increase the wind factor k.

(b) Individual estimates from the isobaric charts probably give the wind speed correct to ± 3 m/sec and the bearing to $\pm 20°$. 40 observations of wind speed and direction were used to obtain a 10-day mean wind vector, and it is to be expected that the spread of this mean will be less than that of individual observations. For instance, if we assume that the speed and angle errors can be treated separately and that the errors are distributed normally, then in the case of speeds, the standard error of the mean will be about 0·25 units. We would then expect that any mean value of speed we computed would have a 95 per cent chance of being within $\pm 0{\cdot}48$ m/sec of the true value. In a similar manner the uncertainty in the computed direction would be $\pm 3{\cdot}2°$.

These estimates are probably over-optimistic and the two inaccuracies are not independent, but the method seems sufficiently accurate in view of the other uncertainties.

(c) The period of time from release to recovery is known as the ' drift time.' It is a maximum value, in the sense that the envelope was not floating and affected by the wind for longer than this time, and the actual drift time may have been less owing to the card not being picked up immediately, but the error would probably not be more than a day or two.

(d) Bowden (1953b) has shown that there is little lag in the production of a steady current by the wind; the direction of the resultant drift vector may oscillate, but the overall mean is likely to be near the final value for the steady state.

(e) According to the theory of stationary wind drift in an open ocean, the angle between the sea-surface wind and the resulting water movement is constant and to the right of the wind in the northern hemisphere (to the left in the southern hemisphere). These relations do not apply near coastal boundaries where there is not generally horizontal uniformity, and consequently gradient currents are present which modify the surface-current regime. Moreover, near coastlines there are generally strong tidal streams which are likely to affect the system (Bowden 1953a) in such a way that the wind factor is reduced. These tendencies localize the application of the ' wind factor ' and ' deviation angle ' to the region in which the observations have been made, and also to the time of year. In this connexion it would be most interesting to compare results from one year to the next.

There is no evidence of well-defined permanent currents in the area, although the large angle values of the southern part of the Bay of Biscay could have been produced by such a flow in a south-westerly direction.

(ii) *Results*

In oceanography it has been the practice to relate the ' sea-surface ' wind, which is by convention the wind that would be measured at a point 10 m above the sea surface, to the resulting surface drift. This is satisfactory where adequate direct wind measurements with anemometers can be made, but for large-scale experiments over the sea sufficient direct observations are not generally available and then wind estimates are best made from isobaric charts. The disadvantage here is that the relation between the gradient and sea-surface winds must be known so that the usual oceanographic relations can be calculated. The ratio of the sea-surface wind speed to the wind at 2,000 ft is roughly 2/3, but there is much controversy with regard to the angle between them. A cross-isobar flow is generally assumed, but the experimental results by Sheppard, Charnock and Francis (1952) from observations in westerly situations near the Scilly Isles indicate that no flow of air from high pressure towards low pressure occurs at the sea surface. This is also confirmed on the climatological charts M.O. 483 showing average surface winds and pressure distribution, where the angle of flow is small in the eastern part of the North Atlantic. On the other hand, over the North Sea a mean angle of 16·5°, which was obtained by Jeffreys (1920), is not much less than that obtained over land. Jeffreys's work is discussed in a paper by Westwater (1943), which quotes evidence from the open ocean that the angle is small. There seems good reason to believe that the inflow angle is a few degrees at most, and possibly zero.

In the present investigation the gradient wind was used since it could be obtained directly from the isobaric charts without recourse to any assumption concerning the relative directions of the gradient and sea-surface winds.

Assuming the surface-wind speed to be two-thirds of that of the gradient wind, the surface-wind factor k_s is 3·3, which agrees fairly closely with other recent estimates. For fairly high wind speeds Thorade (1914) obtained the relation

$$k_s = 1{\cdot}26/(\sin\phi)^{\frac{1}{2}}$$

where ϕ is the latitude, which gives 1·44 in the mean latitude of our area. Rossby and Montgomery (1936), applying formulae from experimental fluid dynamics, developed a theory which gives a wind factor of 2·53 in lat. 50°. Their theoretical results are more likely to apply to the immediate surface of the sea, whereas the Thorade relation, based

on a large number of observations of ships' drift, is more representative of the movement of a layer several metres deep. Ekman's (1953) analysis of current measurements made at a depth of 5 m, from the *Armaur Hansen* in 1930, reveals a wind factor of 1·85. Here again, however, the result refers to a deeper layer than is sampled by the drift cards. Recent observations by Stommel (1954) near Bermuda, using drifting buoys to record the movement near the surface, relative to a deep drogue, give a wind factor of about twice the value expected from Thorade's work. Another experimental agreement is provided in the results of an investigation by Van Dorn (1953) of the stress exerted by the wind on the surface of an artificial pond. The scale of the experiments was different, but the wind factor seems to be approaching a limit of about 3·6 as the depth increases. The drifting objects observed were flakes of paraffin wax, which float close to the surface as do the drift envelopes.

If, as discussed in § 5 (ii), the gradient wind and the wind at 10 m had, during these experiments, almost the same direction, it follows that the drift of the first centimetre or so is in very nearly the same direction as the wind at 10 m. The often quoted theoretical value for wind drift in deep water away from coastal influences is 45°. This angle was obtained by Ekman in 1905 from his theoretical investigations of the effect of the earth's rotation on surface drift, suggested by Nansen's observations of the drift of ice during the voyage of the *Fram*. He was, however, obliged to consider an ocean in which the water was homogeneous, and to assume that the coefficient of eddy viscosity was constant with depth. The Rossby and Montgomery theory predicts an angle of 54°44′, which is also to the right in the northern hemisphere. Experimentally, Gallé, Durst and Stommel obtained values which vary between 25° and 60°, the results of Gallé and Durst being obtained from measurements of ships' drift in open water while Stommel's were obtained from measurements a metre of so down. The difference may therefore be due to these measurements representing the movement of a layer of water much deeper than that sampled by the plastic envelopes.

6. Conclusion

Values for the ratio of the gradient-wind speed to the surface-water speed and for the angle between the gradient wind and the resultant water movement have been obtained by considering the drift of plastic envelopes. Combined results from the two different drift periods indicate that on average the water within 1 cm or so of the surface drifts parallel to the gradient wind with 2·2 per cent of its velocity. Since there is some reason to suppose that during the observations the direction of the surface wind (measured at 10 m) was not very different from that of the gradient wind, it follows that the (shallow) surface drift was parallel to the surface winds. The relation of the speed of the surface-water movement to these observed winds would be roughly 3·3 per cent, which agrees fairly closely with recent drift experiments, but not so well with the large body of experimental results based on measurements of ships' drift. The estimate of the deviation angle between the sea-surface wind and the surface drift is less than that normally observed in the oceans, but it may be true of a shallow-surface layer, the larger deflexions reported by earlier workers being more representative of the drift of a somewhat deeper layer.

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551.582.2 (55)

The climate of Kerman, South Persia

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Summary

The meteorological records and the climate of the Kerman basin are surveyed, with particular reference to wind-power, probable annual precipitation and calculated transpiration, as factors relevant to the agricultural development of the region.

1. Introduction

The town of Kerman (30°21′N, 57°10′E), height 5,600 ft a.s.l., lies at the foot of an isolated rocky crag (rising some 1,300 ft above the town), a few miles from the north-east wall of a V-shaped basin, whose arms run north-west and west. The area is shown diagrammatically in Fig. 1. The main part of the basin is at the foot of the V, and is roughly circular, about 20 mi in diameter; the two arms are about 6 mi in width. The containing walls of the basin are : the Kuh-i-Jupar mountains along the southern edge, 13,000 ft in the centre and 8,000 ft at both ends (all heights above sea level) crossed by passes of 7,500 ft in the south-east and 6,300 ft in the south-west of the basin; the Kuh-i-Badaman, up to 9,600 ft, between the two arms; the Kuh-i-Paiyeh along the north-eastern edge, rising to 13,000 ft. The mountains are steep with no tree cover, and support only scanty vegetation anywhere within a day's journey of any but the smallest settlements. The greater part of the plain is covered by shifting dunes, and areas of hard, sun-baked alluvium where outwash fans have coalesced. Apart from crops, and some cultivated trees, on the outwash alluvium there is little vegetation on the plain.

The site of the station at which the records, quoted in this paper, were made is not known with certainty. Before 1915 the station was probably in the British Consulate compound outside the eastern edge of the town of Kerman, immediately below the crag and sheltered to some extent from north and east winds. After 1937 the station was probably in the town or by the air strip to the west of the town, in either case nearer to the centre of the basin than before, and less sheltered.

Records since 1937 have been kept in terms of the Persian civil calendar, in which the year starts on 21 March and each month starts about the beginning of the fourth week of an English month. All dates are given here in terms of the Gregorian calendar unless otherwise stated.

2. Climatic data

(*a*) *Introductory*

The records given in detail below suggest that the pressure pattern for the region shows no local anomalies, and that the large high-pressure system centred over Central Asia is the most important single feature of the winter pattern, which is modified periodically by the eastward passage of depressions from the Mediterranean basin. By the summer the high-pressure system has been dissipated, so that the main feature of the summer pattern is an area of low pressure extending over Sind, South Persia and Arabia.

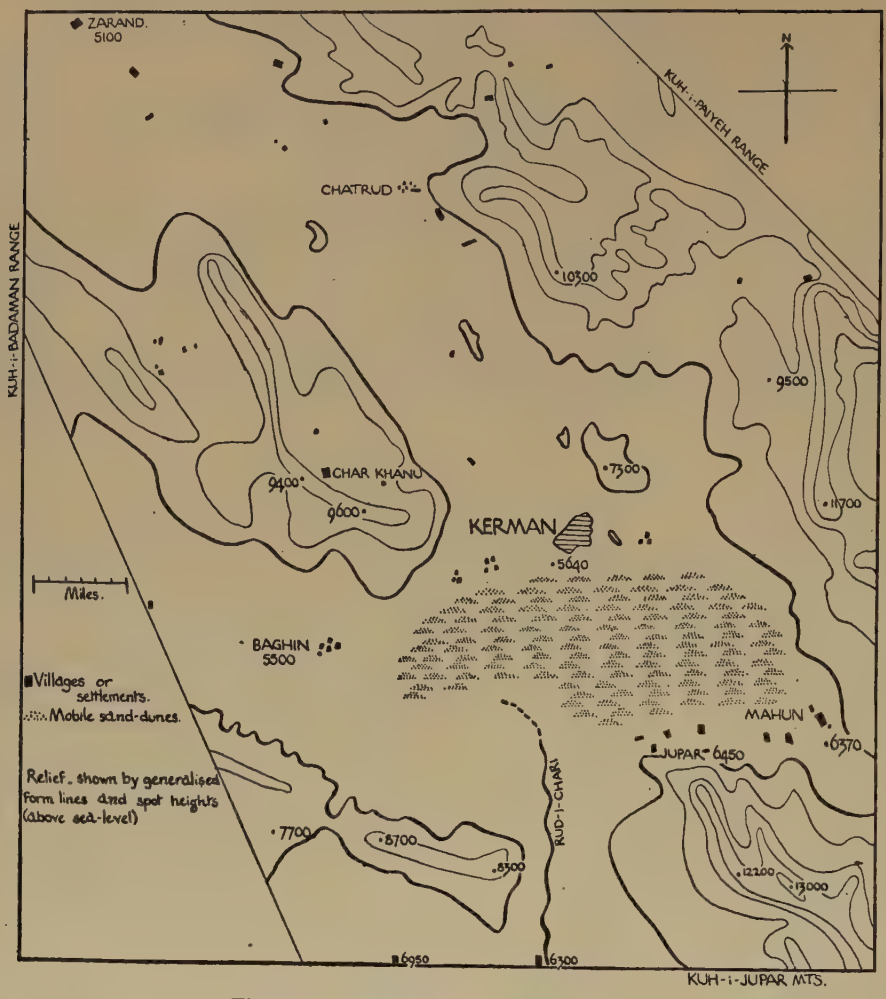

Figure 1. Sketch map of the Kerman basin.

(*b*) *Wind*

Mean daily velocities for the period 1909-15, and mean monthly percentage frequency of wind direction for the period 1911-15, at 08 hr LMT are given in Table 1 and Fig. 2 respectively (iii) (sources of meteorological records are listed under References and referred to here and elsewhere by small roman numerals).

TABLE 1. MEAN DAILY WIND VELOCITY (mi hr^{-1})

J	F	M	A	M	J	J	A	S	O	N	D	Year
3·0	3·3	3·9	3·8	3·2	3·8	3·0	3·4	3·1	2·8	2·8	2·5	3·2

The high proportion of calms recorded may in part be due to the shelter of the surrounding mountains. However, unpublished records for elsewhere in Persia suggest that this is typical of Central Persia, and show also that many of the calms persist throughout the day.

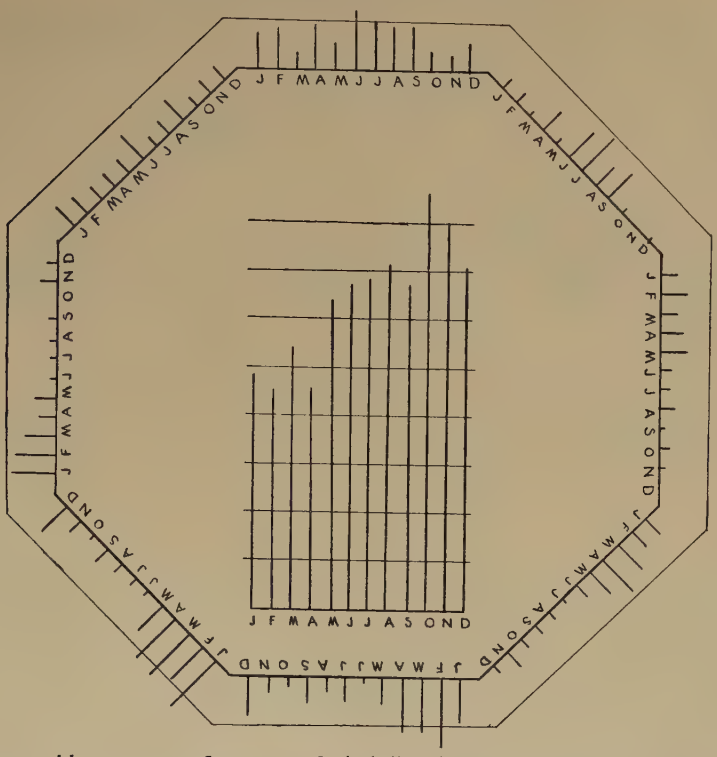

Figure 2. Mean monthly percentage frequency of wind direction at 08 hr LMT. Length of strokes indicates percentage frequencies : parallel lines indicate 10 per cent frequency intervals. Calms in centre of wind rose.

During the course of the year the predominating wind changes in a regular manner. During winter and spring a succession of depressions give rise to periods of strong S and SW winds, accompanied by mild weather and sometimes rain or snow. These winds increase in velocity (reported as being frequently of ' blizzard strength ' by residents), and cause cold weather as they veer to north. Occurrences of this succession continue through March and April, latterly accompanied by dust or sand storms, sometimes very severe – ' the wind-driven sand will scrape the cellulose paint from a car, leaving the steel shining like aluminium, and will frost the windows so that it is impossible to see through them ' (Oddy 1954). Between such periods the predominant wind is N to NW, but calms are frequent, punctuated by cold katabatic winds down the mountain valleys, particularly from the Kuh-i-Jupar massif.

In summer and autumn the prevailing winds are N and NE, but the general impression is that calms predominate, rising from nearly 70 per cent of records at 08 hr LMT in the summer to 85 per cent in the autumn, with afternoon gustiness and dust-devils, due to local heating, on the plain. On an average there are 3 (up to 12) days between July and early September of strong S or SW winds and dust storms.

(c) *Temperature*

Mean daily maximum and minimum temperatures for the periods 1909-1915 (ii) and 1941-50 (i) (records for 1941-50 in terms of Persian calendar), and absolute monthly maximum and minimum temperatures, for the period 1909-15 (ii) are shown in Table 2 and Fig. 3, all measured at 08 hr LMT. The absolute annual maximum and minimum for the period March 1941-March 1952 were 112°F and 3°F respectively (iv), comparable

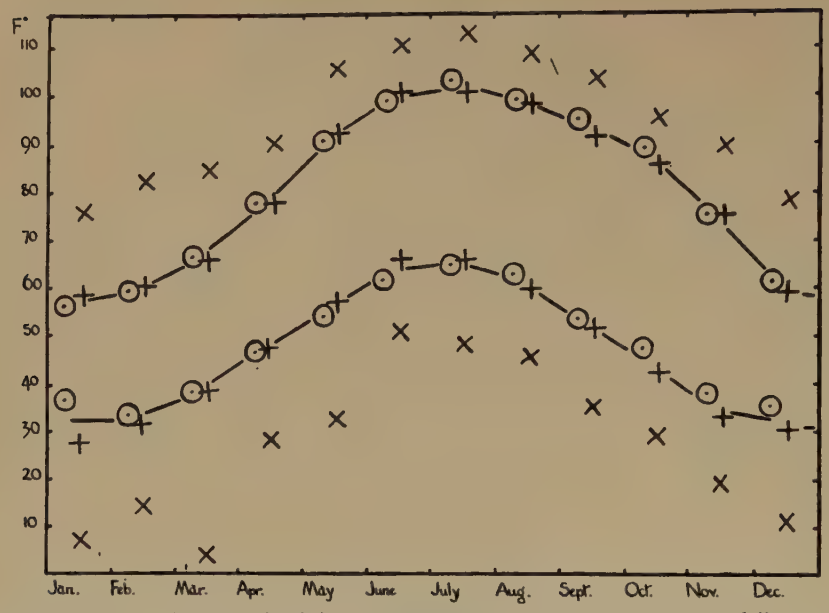

Figure 3. Mean daily maximum and minimum temperatures. ⊙ — ⊙ — ⊙ mean daily maximum and minimum (1941-50); + — + — + mean daily maximum and minimum (1909-15); × × × absolute monthly maximum and minimum (1909-15).

TABLE 2. MEAN DAILY MAXIMUM AND MINIMUM TEMPERATURES (°F) at 08 hr LMT

1909-1915	J	F	M	A	M	J	J	A	S	O	N	D
Max.	58	60	65	77	91	101	101	98	92	85	73	58
Min.	27	31	38	47	56	65	65	59	51	42	33	30
1941-1950	Dey	Bah.	Esf.	Farv.	Ord.	Khor.	Tir	Mord.	Shah.	Mehr	Aban	Azar
Max.	57	59	67	76	91	99	104	99	95	89	75	62
Min.	35	34	38	46	54	61	64	62	54	47	38	35

Each Persian month commences about one week before its English counterpart

with 112°F and 5°F for the period 1909-1915. An even lower figure, of ' over 40° ' (of frost) is reported for the Narmashir district of Bam, to the south of Kerman, in February 1905 (H.M. Consul's report). There appears to be a small consistent difference between the monthly means of the two periods, greater than that due to the fact that monthly means from records based on the Persian calendar are for periods starting one week earlier. The difference is probably due to the change in the position of the instruments : the diurnal range at the more recent station appears to be very slightly less than that at the earlier. The very low absolute minimum for March does not appear to be due to an incorrectly transmitted record; compare for example the following paraphrased extract from H.M. Consul's report for 1928 :

February : Frost and wind severe.

March : Sheep are dying in large numbers. Fruit trees and crops damaged by frost.

In a typical year the weather of the first half of May is felt to be cold, and the second half warm, and the reverse in September. Between these the hottest month is usually

July. In an average year the annual succession of the seasons starts at *No Ruz* (21 March, the Persian New Year) with a short cool spring, followed by a long hot summer (mid-May to mid-September), a short cool autumn (mid-September to mid-November) and a long cold winter (four months to mid-March), showing the extreme variation expected of a plateau climate.

Table 3 compares mean temperatures in the summer of 1950 between Jupar (6,500 ft) on the edge of the plain at the foot of the Kuh-i-Jupar, and Char-Khanu (7,000 ft) in one of the high valleys of the Kuh-i-Badaman. The figures emphasize the lower daily mean, and the greater diurnal variation, in the mountain valleys than on the plain.

TABLE 3. DIURNAL TEMPERATURE VARIATION IN SUMMER 1950

	Height (a.s.l.)	Period	0430 hr LMT	0600 hr LMT	1400 hr LMT
Jupar	6,500 ft	3 weeks August	*c.* 58°F	71°F	83°F
Char-Khanu	7,000 ft	1 week August/September	37°F	49°F	78°F

Frosts do not normally begin before the end of October, and are usually over by the end of March, though freak frosts can occur during April and May. Katabatic winds from the mountains often cause sharp changes in temperature during the cold months.

(*d*) *Precipitation*

The considerable variation in precipitation from one year to another is clearly shown by the monthly totals for 1910-1915 in Table 4 (a) (vi) and 1941-1950 in Table 4 (b) (i).

All available records for total annual precipitation are given in Table 5. Since most of the annual totals are for the Persian year (beginning 21 March), the totals for 1912-1915, obtainable from Table 4, are calculated for the year 1 April-31 March. The discrepancy between the total for 1911-12, referred to the Persian calendar, recorded in the report of H.M. Consul at Kerman, and a total of 4·42 in. calculated from Table 4 for the period 1 April 1911—31 March 1912 serves to emphasize the dangers of comparing totals for periods that are not identical.

It is worth pointing out that from an agricultural point of view the Persian calendar year, with which nearly all the wet season comes in one year instead of being bisected in mid-winter, provides a very much better basis for estimating total annual precipitation than does the Gregorian calendar, though in fact the rainfall affecting each agricultural year is that of the preceding Persian calendar year. The range of annual totals (3·2 in. to 9·0 in.) of Table 5 demonstrates clearly the marked variations in the amount of water available for the main cereal crops, sown in the autumn, much more so than the annual totals of Table 4. For example the very low total for 1912 (Table 4 (a)) obscures the fact that a heavy autumn fall in 1911 and a heavy spring fall in 1913 provided sufficient precipitation to bring the total water available for winter crops harvested in 1912 and 1913 very nearly to what it would be in an average year. Apart from very rare monsoon rain, mentioned below, precipitation in the area is entirely derived from the depressions of winter and spring, so the seasonal distribution and the variation in annual totals of rain and snow are closely related to the occurrence of depressions and the associated sequences of mild damp, followed by cold dry, weather.

TABLE 4. MONTHLY PRECIPITATION (INCHES)

(*a*) *1910-1915*

Figures in brackets show heaviest 24-hr rainfall during the month; rain-days are ' probably number of days with more than 0·1 in. of rain ' (ii)

	1910	1911	1912	1913	1914	1915	Mean	Mean No. of rain-days
Jan.	–	1·09 (0·26)	0·16 (0·04)	0·56 (0·25)	0·50 (0·19)	0·17 (0·17)	0·5	2·4
Feb.	–	0·54	0·0	0·57 (0·31)	2·46 (0·62)	–	0·9	2·5
Mar.	–	0·83 (0·28)	0·17 (0·08)	1·94 (0·49)	0·35 (0·14)	0·94 (0·50)	0·8	2·6
Apr.	–	0·35 (0·14)	0·87 (0·42)	0·28 (0·19)	1·08 (0·32)	1·14 (0·78)	0·7	2·2
May	–	0·35 (0·15)	0·0	0·0	0·0	0·33 (0·27)	0·1	0·6
June	–	0·0	0·0	0·0	0·89 (0·40)	0·0	0·2	0·6
July	–	0·0	0·0	0·0	0·0	0·0	0·0	0·0
Aug.	–	0·01 (0·01)	0·0	0·0	0·0	0·0	0·0	0·0
Sept.	–	0·0	0·0	0·03 (0·02)	0·21 (0·12)	0·0	0·1	0·2
Oct.	–	0·01 (0·01)	0·0	0·15 (0·15)	0·16 (0·10)	0·0	0·1	0·4
Nov.	0·05 (0·03)	1·28 (0·58)	0·0	0·99 (0·78)	0·76	0·0	0·5	1·3
Dec.	2·38 (0·54)	2·09 (0·60)	0·88 (0·44)	1·54 (0·34)	0·33 (0·22)	–	1·4	5·6
Year	–	6·55	2·08	6·06	6·74	–	5·3	18·4

(*b*) *1941-1950* (*Persian calendar*)

	1941	1942	1943	1944	1945	1946	1947	1948	1949	1950	Mean
22 Dec. – 20 Jan.	–	0·47	1·18	2·32	0·58	2·12	1·61	0·79	0·34	0·0	1·05
21 Jan. – 19 Feb.	–	3·78	0·79	0·0	0·83	0·68	0·12	1·59	1·40	0·65	1·09
20 Feb. – 20 Mar.	–	1·46	2·99	0·47	0·22	0·48	2·0	0·47	2·85	0·30	1·25
21 Mar. – 21 Apr.	2·79	0·39	0·14	0·0	1·72	0·20	0·75	1·93	1·22	–	1·02
22 Apr. – 21 May	0·20	0·35	0·43	1·48	0·55	0·08	0·0	0·24	0·0	–	0·37
22 May – 21 June	0·10	0·0	0·0	0·0	0·0	0·0	0·0	0·0	0·0	–	0·01
22 June – 22 July	0·0	0·0	0·0	0·0	0·0	0·0	0·0	0·0	0·0	–	0·0
23 July – 22 Aug.	0·0	0·0	0·0	0·0	0·0	0·0	0·12	0·0	0·0	–	0·01
23 Aug. – 22 Sept.	0·0	0·0	0·0	0·0	0·0	0·0	0·0	0·0	0·0	–	0·0
23 Sept. – 22 Oct.	0·0	0·0	0·63	0·0	0·0	0·0	0·0	0·0	0·0	–	0·07
23 Oct. – 22 Nov.	0·0	0·0	0·06	0·0	0·10	0·0	0·0	0·0	0·0	–	0·02
23 Nov. – 21 Dec.	0·20	0·0	0·0	0·89	1·15	1·05	0·0	0·32	1·02	–	0·51
Year (22 Dec. – 21 Dec.)	–	6·45	6·22	5·16	5·15	4·61	4·60	5·34	6·83	–	5·4

TABLE 5. TOTAL ANNUAL PRECIPITATION (PERSIAN CALENDAR)

Year 21 March–20 March	Precipitation (in.)	Source
1909–1910	5·6	(v)
1910–1911	4	(v)
1911–1912	5·59	(v)
1912–1913	4·82	(vi) (1 Apr.-31 Mar.)
1913–1914	6·30	(vi) (1 Apr.-31 Mar.)
1914–1915	5·44*	(vi) (1 Apr.-31 Mar.)
1937–1938	7·6	(iv)
1941–1942	9·00	(i)
1942–1943	5·70	(i)
1943–1944	4·05	(i)
1944–1945	4·00	(i)
1945–1946	6·80	(i)
1946–1947	5·06	(i)
1947–1948	3·72	(i)
1948–1949	7·08	(i)
1949–1950	3·19	(i)
1950–1951	8·7	(iv)
1951–1952	7·2	(iv)

Mean annual precipitation : 5·89 in. (excluding 1909-11)

* 1914–15. For eleven months recorded plus mean for February.

In a good year the first heavy showers, and possibly snow in the mountains, might fall at the end of November. The beginning of December would be relatively dry, but by the end of the month the expected annual fall of snow and rain should be well begun, with snow falling over the whole of the plain. Intermittent rain and snow continues through January and February with sensible, if insignificant, precipitation up to one to two days in the week.

Snow does not normally fall after the middle of February, but freak falls have occurred as late as April. The snow on the mountains has mostly thawed by the end of May. In Kerman even today, snow from the mountains is stored in underground ' ambars ' through the summer, as the basis of various cool drinks sold in the bazaar. As stored it probably provides a potent carrier of infection for most of the stomach diseases endemic in the Middle East.

Heavy showers with bright intervening periods continue in March, and ' short afternoon showers ' in April (Oddy 1954). There are occasional showers in May and then no further rain until the autumn, except for a possible but rare cloudburst in July or August.

The short period of strong S to SW winds in late July and August sometimes brings heavy sultry weather; note the increase in cloud amount and relative humidity (Tables 7 and 8). In 1950, for example, there was a period of some 4-5 days during which the (unmeasured) humidity apparently rose and heavy clouds came up from the south – this ended on 3 August with a moderate fall of warm heavy raindrops that evaporated as soon as they reached the ground. Occasionally there is a thunderstorm, which may be catastrophic : during the fourteen years 1911-15, 1941-50 there is only one record of appreciable rainfall in July or August (0·12 in. in 1947), but in 1932 there was a very severe cloudburst ' with thunder, floods, people drowned, collapsed buildings, cut roads, ruined *qanats* – in July ! ' (Oddy 1954), of which the centre of damage was Mahun, where the valley leading to the south-east pass debouches onto the plain. It would seem that this short period of modified weather and southerly winds probably marks the extreme limit

inland of the influence of the Indian Monsoon. Table 6 gives rainfall figures for Isfahan 1893-1940 (vii, viii, ix). The figures might be taken to suggest that here also there are rare but relatively heavy falls in July. In 1932 the July rainfall for Isfahan was 2·02 in.

TABLE 6. SUMMER RAINFALL FIGURES FOR ISFAHAN

	June	July	August
Number of years in 47 in which there was rain	14	4	4 (in 46 yr)
Mean monthly rainfall for years with rain (in.)	0·15	0·71	0·14

The small number of rain-days (Table 4) in each month and the bracketed figures for heaviest rainfalls in each month serve to emphasize a general impression that showers of high intensity are not uncommon. There is much evidence of heavy erosion, in the form of gulleys cutting into alluvial fans, in the alluvial fans themselves, which form a wide belt round the plain, and in numerous reports of floods. Some villages are crossed by wide dry river beds (used as roads in summer) to carry off flood water in spring and winter. Of the years for which there are records, 1909 and 1910 appear to have been particularly catastrophic; in 1909 floods did considerable damage at Mahun; in 1910 heavy late rain in the spring caused a great flood on the spurs of the Kuh-i-Lalezar (south of Kuh-i-Jupar) destroying nearly 150 villages and causing 4,000 people to leave their homes, and another flood caused devastation at Zarand, Rawar and Chatrud (in the north-west arm of the V). Most large villages are sited at the mouths of wide mountain valleys, to draw on the relatively strong flow of ground water through the erosion debris filling the valley bottoms, and are therefore well placed to suffer maximum damage from the heavy and rapid run-off from nearly bare mountains.

(*e*) *Cloud*

The average cloud amount at 08 hr LMT for the period 1908-15 is given in Table 7 (ii).

TABLE 7. AVERAGE CLOUD AMOUNT (TENTHS)

J	F	M	A	M	J	J	A	S	O	N	D	Year
2·6	2·9	3·0	1·8	0·3	0·3	0·5	0·1	0·6	0·3	1·1	3·1	1·4

The figures for the winter months correspond fairly closely with those for rain-days and rainfall for 1911-15; the December average would probably be lower over a longer period. Comment has already been made on the small increase in July.

(*f*) *Humidity*

Mean monthly relative humidities and vapour pressures at 08 hr LMT for 1908-15 are shown in Table 8 (ii).

TABLE 8. MEAN MONTHLY HUMIDITY

	J	F	M	A	M	J	J	A	S	O	N	D	Year
Relative humidity (per cent)	63	58	58	49	35	30	34	33	41	47	51	67	47
Vapour pressure (mb)	4·6	4·4	5·9	7·5	8·5	10·4	11·8	9·1	7·7	6·2	4·8	4·9	7·1

3. APPLICATIONS OF DATA

The above records were collected as part of the work of the Oxford University Expedition to Persia, 1950, to provide a descriptive background to its geographical studies, and the answers to some problems relevant to further agricultural development in the Kerman basin, namely :

(i) *Are the winds strong and consistent enough for water to be raised more cheaply in the dry season by windpumps than by the local ' qanat ' irrigation works ?*

No definite answer is possible on the evidence available. As an extremely crude approximation to actual wind velocity, the figures in Table 9 may be calculated from mean wind velocities and the percentage of calms.

TABLE 9. CALCULATED WIND VELOCITIES AT 08 HR LMT ON DAYS WITH WIND

	M	A	M	J	J	A	S	O	N
Number of days with wind	14	16½	11	10	10	9	10	4½	6
Mean wind velocity (mi hr^{-1})	8·5	6·9	8·9	11·5	9·4	11·7	9·4	20	14

The power available from a given area of vane is proportional to the cube of the wind velocity, so that it would be necessary to know the relative frequency of winds of different velocities in order to solve the problem (Golding 1953). Table 9 suggests fairly simply, even admitting that there might be more frequent and stronger winds later in the day than 08 hr LMT, that the power available would probably not be sufficient to raise significant amounts of water without considerable capital investment in vanes and pumps. A very rough extrapolation, from the figures given by Golding (1953, p. 597), suggests that the capital cost of wind-driven pumps for a given supply of water might be as much as five to ten times the cost of *qanats*, expensive though the latter are (Beckett 1953). Kerman lacks the consistent ' 120-days wind ' of Seistan and it is probably significant that none of the traditional Persian windmills are reported from or were seen in the Kerman area.

(ii) *What annual precipitation may the cultivator reasonably expect, bearing in mind the considerable year-to-year variation ?*

Taking the 16 annual totals between 1911 and 1952 given in Table 5 we have :

Mean = 5·89 in.
Standard deviation = 1·74 in.

Assuming, to a rough approximation, that the annual totals may be described by a normal frequency distribution (Glover and Robinson 1953), and using a table of the probabilities of deviates as on a normal curve, the probabilities that the annual total will exceed the following values may be calculated as in Table 10.

TABLE 10. PROBABILITIES OF ANNUAL PRECIPITATION (IN.) EXCEEDING PARTICULAR VALUES

Precipitation	3	4	5	6	7	8
Probability	0·95	0·86	0·70	0·47	0·26	0·11

It is of interest to note the reasonably close agreement of the relative standard deviation of the annual precipitation of Kerman, which is 0·29, with the corresponding figure for Shiraz for the 29 years 1923-52 (Persian calendar) (ix) which give :

Mean = 13·22 in.
Standard deviation = 3·49 in.
Relative standard deviation = 0·26 in.

The standard deviation of a rainfall is a statistically useful measure of the extent to which individual figures tend to deviate from their mean, and in this case is a measure of the variability of annual precipitation. Expressed as a fraction of the mean the standard deviation becomes the relative standard deviation (R.S.D.). It is found that in many cases samples with widely varying mean values and S.D's but drawn from a single population, show fairly constant R.S.D. (Snedecor 1946). In the absence of evidence to the contrary the similarity between the R.S.D. of annual precipitation for Kerman and Shiraz suggests that the two sets of figures might be considered as two samples from the same population, and that the precipitation for both places is probably derived therefore from the same sequence of climatic conditions. This is in agreement with climatological evidence that nearly all the precipitation at both Shiraz and Kerman comes from Mediterranean depressions moving eastwards. While the agreement between the calculated R.S.D. may be fortuitous in this case – both samples are small – and deductions from the agreement add nothing to our knowledge of the climate of the area, it would be of interest to compare them with similar figures for stations on the south-east coast, Caspian coast and in the extreme north-east of Persia, for which equally or more important sources of precipitation are the Indian monsoon, onshore winds from the Caspian, and depressions moving E from the Black Sea respectively.

(iii) *What are the potential losses of water by evaporation from reservoirs and transpiration from crops ?*

Potential losses were calculated by Thornthwaite's formula (1948) for total evapotranspiration, and by Penman's method (1948) for open water surfaces and for areas completely covered by crops. Results are given in Table 11. The meteorological data are not very adequate for the latter method : arbitrary constants have been interpolated where necessary, and the calculated transpiration from crops has been reduced by a factor of 0·85 for all months to make some correction for partial closing of the stomata of the transpiring vegetation (Penman 1948, Russell 1950). It has also been assumed that the reflection coefficient of the vegetation is 0·2.

The figures for potential evapotranspiration and transpiration from a continuous crop cover are of the same order and probably provide a reasonable basis for estimating irrigation requirements. In either case these figures, with those of Tables 5 or 10, make it clear that all crops must be irrigated, as indeed is the case : even in the high valleys there was no evidence of any dry-farming, though the cultivators may grow a few quick-growing melons without irrigation, but only locally on low-lying ground, and after a heavy fall of late spring rain.

TABLE 11. CALCULATED POTENTIAL MONTHLY EVAPORATION OR TRANSPIRATION (IN.)

Month	Potential evapotranspiration (Thornthwaite)	Evaporation from open water surface (Penman)	Transpiration from continuous crop cover (Penman)
Jan.	½	1½	1
Feb.	½	2½	1
Mar.	1	3½	2½
Apr.	2½	5½	4
May	5	7	5
June	7	8½	5½
July	7	8	5½
Aug.	6	7	5
Sept.	4	5½	4
Oct.	2½	3½	2½
Nov.	1	2	1
Dec.	½	1	1
Total	36½	55½	38

The agricultural year is governed by the balance of precipitation and transpiration. The most important food crops, the winter cereals, are sown in the late autumn and begin to ripen in early summer, to be harvested in mid-summer. Periods of maximum growth coincide with the early 'winter rains' and the 'spring rains,' and the water-holding capacity of the soil is sufficient to retain the balance of the winter precipitation until the spring growth begins – the existence of horizons of accumulation of calcium carbonate and sulphate at relatively shallow depths in well-drained uncultivated soils (zonal soils) demonstrates that the annual replenishment of the soil water is insufficient to cause drainage out of the root zone. Irrigation alone, from *qanats* and the few perennial streams, is unable to provide sufficient water to meet all crop requirements at these periods, if the normal acreage (barely sufficient to provide work, and therefore subsistence, for the agricultural population) is sown. If one or other of the winter or spring rains fails the consequences may be serious; if both fail or are seriously inadequate then the local situation becomes desperate and there is a likelihood of starvation in the villages and also great loss of stock.

Autumn crops, mainly minor grains and vegetables, are sown after the hottest part of the summer, to be harvested in November. During June to August the only crops in cultivation are vegetables, with melon, sunflower, castor oil, and fruit trees, all irrigated frequently.

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Sources of meteorological records

(i) Manuscript copy of local weather records provided by the Provincial Governor-General's office in Kerman.
(ii) Unpublished data from the Meteorological Office, Harrow.
(iii) Data from Indian records, provided through the U.K. High Commissioner in New Delhi.
(iv) Data read off a manuscript graph and histogram provided by the Ministry of Agriculture, Tehran, through H.M. Embassy.
(v) Diplomatic and Consular reports for Kerman (1898-1912).
(vi) Government of India Monthly Weather Review (1910-1915).
(vii) *World Weather Records*, Smithsonian Miscellaneous Collection, Volume **79** (1927).
(viii) *Ibid.*, Volume **90** (1934).
(ix) *Ibid.*, Volume **105** (1942).
(x) Unpublished figures provided by the Rev. N. Sharp, C.M.S. Mission, Shiraz.

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The measurement and interpretation of near-surface sea-temperature profiles

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SUMMARY

Preliminary measurements of the temperature distribution in the topmost three metres of the open ocean have been made from an Ocean Weather Ship. The two profiles recorded are discussed in relation to the important problem of heat transfer near the sea surface.

1. INTRODUCTION

Although measurements of the near-surface temperature gradient in lakes and bays have frequently been reported, detailed observations from the open ocean are rare. In view of the importance of such measurements in marine meteorology and oceanography a suitable instrument was designed and tested at sea in the O.W.S. *Weather Observer* (52°50′N, 19°40′W) in the summer of 1950. This note describes the instrument and discusses the two profiles which were recorded.

2. THE INSTRUMENT

Eight sets of copper-nickel thermocouples were mounted at intervals in a 12-ft brass tube, with a common reference junction at the bottom. The whole was sealed and weighted to float as near to the vertical as possible. The current produced by the thermal e.m.f. due to the temperature difference between any one of the junctions and the reference was read on a marine galvanometer mounted on board ship. The junctions were all mounted in half-inch lengths of ebonite rod which fitted into three-inch lengths of brass tube brased into the main tube. The wax in which the junctions were embedded gave them a lag in response to changes of temperature of between 10 and 15 min. This caused effects such as the motion of the instrument in the water to be smoothed out. The amplitude of the up-and-down motion of the instrument was observed to be about 3 in. The possibility of appreciable conduction along the thermocouple wires was removed by coiling $1\frac{1}{2}$ to 2 in. of the wires at the level of each junction. Further instrumental details are not included here because substantial modifications would be advisable if such an experiment were to be repeated.

The apparatus was floated only in light to moderate winds when the ship was hove-to. The mean angle of tilt of the instrument was observed to be about 15° and the depths have been corrected accordingly. The temperature differences are mean values from the several galvanometer readings which were necessary because the coil was not perfectly balanced and swung through about 1 cm (equivalent to 0·2°F) as the ship rolled. The standard deviations of the individual readings are all about 0·06°F.

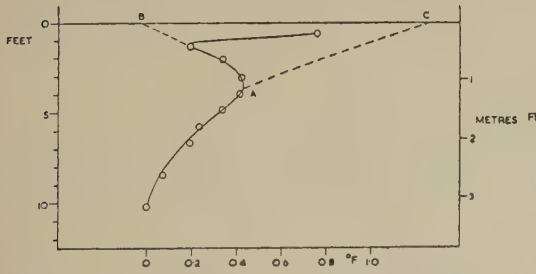

Figure 1. Profile 1, 28 July 1950, 1430 GMT. Temperature versus depth with lowest point as reference temperature. Full line, observed profile; pecked line, extrapolations.

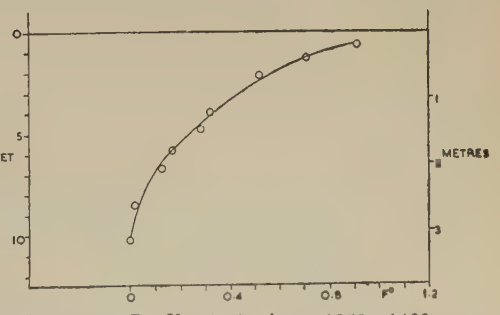

Figure 2. Profile 2, 7 Aug. 1950, 1430 GMT. Temperature versus depth with lowest point as reference temperature.

* Now at Meteorological Office, Dunstable, Beds.

A first field experiment of this nature is rarely without its unforeseen problems. The one most difficult to overcome was the behaviour of the apparatus as a sea-anchor dragging in the wake of the ship as she drifted in the wind. The efforts of two men in a dinghy were necessary to keep the apparatus in clear water. As a result only two profiles were successfully recorded; these are shown in Figs. 1 and 2 and the relevant weather observations are given in Table 1.

TABLE 1. WEATHER CONDITIONS DURING THE OBSERVATIONS

Date	Time (GMT)	Temperature (°F) Dry bulb	Wet bulb	Sea bucket	Cloud	Wind (kt)	Swell
28 July 1950	0600 to 1000	56·0	55·0	57·8	8/8 Sc 2,000 ft	1	
28 July 1950	1000 to 1345	56·5	55·0	58·0	7/8 Sc 1,500 ft 4/8 St 500 ft	10	
28 July 1950	1345 to 1430	58·5	55·0	58·0	2/8 Sc 2,000 ft	12	Low, period 10–15 sec
7 Aug. 1950	0600 to 1100	55·5	51·0	57·2	7/8 Sc 4,000 ft 6/8 Sc 2,500 ft	12	
7 Aug. 1950	1100 to 1430	57·5	48·5	57·2	7/8 Cu Sc 3,500 ft 3/8 Cu 2,000 ft	10	

3. DISCUSSION OF THE PROFILES

If one neglects heat which is advected into the water column under consideration, the dominant terms in the equation expressing its energy budget are : (a) the amount of heat added by radiative processes, (b) the amount of heat removed by turbulent exchange processes in the air, and (c) the change in heat storage.

Crude estimates of the magnitude of the first two terms are made using existing empirical methods. Their sum is then compared with an estimate of the third term made using the observed sea-temperature profile on the not unreasonable assumption that at sunrise the temperature near the surface is uniform. A plausible value of this uniform temperature is indicated on each profile.

The amount of heat added by radiative processes is estimated using the technique described by Sverdrup (1941) and the data of Table 1. The incident short-wave radiation, suitably corrected for scattering and cloudiness, and summed over the period from sunrise to 1430 GMT, is 190 cal cm^{-2} on 27 July and 150 cal cm^{-2} on 7 August. Effective back radiation, estimated using Ångström's method is 14 cal cm^{-2} and 19 cal cm^{-2} respectively. Assuming an albedo of 0·08, and 77 per cent absorption of transmitted radiation in the top three metres, the heat absorbed in the column of water is 120 cal cm^{-2} on 27 July and 100 cal cm^{-2} on 7 August. This estimate may be considerably in error.

Transfer of heat from the ocean to the atmosphere by turbulent-exchange processes can conveniently be estimated using a method outlined by Montgomery (1948). This is based on the difference between the sea-surface and the wet-bulb temperatures and an assumed aerodynamic roughness. Sensible heat transfer and evaporation are not separated. The relevant expression from Montgomery's paper for the unit-area eddy flux of heat is

$$\left(\int_a^b \frac{dz}{K\bar{\rho}}\right)^{-1} c_{pd}\,(\bar{T}_{ea} - \bar{T}_{eb})$$

where dz is an element of height, integrated in this case from the sea surface to the height of the ship's screen, $\bar{\rho}$ is mean air density, c_{pd} is specific heat of dry air at constant pressure and

$\overline{T}_{ea}$ and $\overline{T}_{eb}$ are the equivalent temperatures at the two levels. The latter are related to the wet-bulb temperature T, latent heat L and mixing ratio q according to the equation

$$T_e = T + \left(\frac{L}{c_{pd}}\right) q$$

For the eddy diffusivity, K, the expression $\dfrac{u\,k^2\,z}{\log_e z_b/z_0}$ is substituted; u being the wind speed at height z_b, and k von Kàrmàn's constant, for which a value of 0·4 is used. For the aerodynamic roughness z_0 we use the value of 0·02 cm following Sutton (1949). The observations of Table 1, assumed appropriate to a height of 7 m, then indicate that the total amount of heat lost from the column to the atmosphere by turbulent exchange is 31 cal on 28th July and 71 cal on 7 August. Again, this estimate can be considerably in error, as can an estimate of the change in heat storage in the column which is calculated as 75 cal and 56 cal respectively against 89 cal and 29 cal from the previous figures. Nevertheless a rough balance has been achieved and the measured profiles are therefore not unreasonable.

Having decided that the total change in heat storage is of the right order we can go on to discuss the form of the profiles. We can perhaps make reasonable assumptions regarding the heat which is absorbed into the column at given levels and given times. Then the form of the profile would be determinate in terms of the turbulent characteristics of the surface layers of the ocean. However, little is known of oceanic turbulence. Various values of the eddy-transfer coefficient are suggested in the literature but there is little information as to how they vary with wind speed, stability or depth. We cannot therefore determine the form of the profiles with any precision : nevertheless they show significant differences and it is tempting to relate them to the different meteorological conditions which existed in the hours before they were recorded.

It is probable that the difference in form of the two profiles is primarily due to the differences in wind strength and resultant mechanical mixing of the sea surface. Also the turbulent flux of heat away from the sea surface is approximately twice as great on 7 August as on the earlier occasion, so we would expect some difference in the intensity of thermal mixing.

Fig. 1 suggests that the cooling by upward turbulent heat flux has been confined to the top metre, while heating by radiation has been virtually unhindered below 1 m. The observed temperature rise in this lower layer agrees with an estimated rise based on values for the incoming radiation quoted earlier and accepted values of absorption coefficients (Sverdrup 1941). Two extrapolations have been made in Fig. 1 as illustration: A B represents the profile which would probably have been observed if there had been no upward flux of heat throughout the period, and A C the profile which would probably have been observed just before the dispersal of cloud at 1345 GMT. The sharp reversal of gradient near the surface may be attributed to the increased heating during the short period of direct solar radiation immediately before the profile was recorded.

Profile 2 is more straightforward and suggests a balance between heating and cooling processes being effected throughout the layer as a result of increased mechanical and thermal turbulence in the sea surface.

Further work on this important problem would need repeated temperature profiles. This note has shown that suitable apparatus can be constructed and used at sea, and it is hoped that further measurements will be made.

Acknowledgments

I am grateful to Professor P. A. Sheppard for his advice and encouragement during the course of this work, to the Marine Superintendent of the Meteorological Office and his staff for their helpful co-operation, and to the Admiralty Research Laboratory for the loan of apparatus.

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551.508.2 : 536.42 : 536.53

The melting point of diphenyl ether as a self-maintaining reference temperature in an equatorial climate

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(Manuscript received 12 May 1956)

SUMMARY

It is possible, and convenient, in an equatorial climate to use the melting point of diphenyl ether (26·6°C) as a thermocouple reference temperature which, under specified conditions, will be self-maintaining in the open air to within ± 0·1°C for periods of about a fortnight, the ambient temperature range being ± 5°C.

1. INTRODUCTION

In a climate with a predominant diurnal variation it is possible to select a fixed temperature (e.g., a melting point) which is self-maintaining for long periods, with the obvious advantages of labour-saving, constancy and simplicity.

2. THEORY

Consider a vessel of surface area αl^2, volume βl^3, containing a solid melting at θ_0, the heat of fusion being L. Suppose the vessel is shielded from radiation and that the air temperature is θ, and its velocity V. Heat will enter the vessel at a rate depending on the surface area, the temperature difference and the air velocity in such a way that

$$dH = q\alpha l^2 V^{\frac{1}{2}} (\theta - \theta_0)\, dt, \quad . \quad . \quad . \quad . \quad . \quad (1)$$

say, where q is a constant factor.
Over an extended period T a mass m of solid will be melted, given by

$$\int_0^m L\, dm = \int_0^T q\alpha l^2 V^{\frac{1}{2}} (\theta - \theta_0)\, dt \quad . \quad . \quad . \quad . \quad (2)$$

If a fraction γ of the substance is initially solid, the fixed point will be maintained as long as

$$\int_0^T V^{\frac{1}{2}} (\theta - \theta_0)\, dt < \frac{L\gamma m}{q\alpha l^2}$$

$$\text{i.e.,} \qquad < \frac{L\gamma\beta l}{\sigma q\alpha} \quad . \quad . \quad . \quad . \quad . \quad (3)$$

where σ is the specific volume of the lighter phase. If I is the average 24-hr value of the integral, the temperature will remain constant for a number of days

$$n = \frac{L\gamma\beta l}{I\sigma q\alpha} \quad . \quad . \quad . \quad . \quad . \quad (4)$$

or, if I is negative,

$$n = \frac{L(1-\gamma)\beta l}{I\sigma q\alpha}$$

The value of I depends on that of θ_0, and, if $|I|$ is reduced by a suitable choice of the fixed point, then n can become quite large. The remainder of the fraction is capable of relatively little variation.

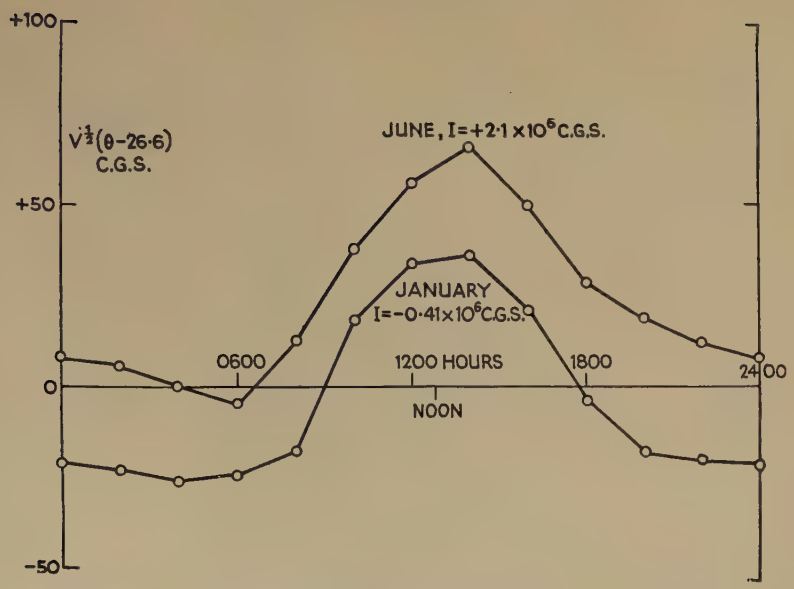

Figure 1. The average daily variation of the heating function $V^{\frac{1}{2}}(\theta - \theta_0)$ for melting diphenyl ether during the months of January and June in Singapore.

3. Suitability of diphenyl ether

Several of the calorimetric properties of diphenyl ether have been precisely determined by Furukawa, Ginnings, McCoskey and Nelson (1951), who also suggested that it would be useful as a thermostatic substance. Quoting this authority, the triple point is 26·87 ± 0·01°C and the heat of fusion 24·176 ± 0·003 cal/g. The specimen used melted at 26·6°C, as compared with a calculated melting point of 26·90°C. The measured point was apparently unaffected by thorough mixing with water, and is assumed to be that of a saturated solution of water in diphenyl ether. The specific volume at 28°C was measured as 0·938 cm^3/g, and the contraction on freezing as 0·091 cm^3/g.

If the above-mentioned linear dimension, l, is the diameter, the value of β/α is 0·17 for a sphere, 0·25 for a long cylinder, and rather less, say 0·1, for a container of a more complicated shape. For stoppered, double, wide-necked vacuum flasks $q \simeq 5 \times 10^{-6}$ cgs. Using these data, a favourable value of the fraction

$$\frac{L\gamma\beta l}{\sigma q \alpha} = \frac{24{\cdot}2 \times 1 \times 0{\cdot}2 \times 6}{0{\cdot}938 \times 5 \times 10^{-6}} = 6{\cdot}2 \times 10^6 \text{ cgs}$$

It is evidently necessary that I should be about 10^6 cgs, or less, if n is to have a useful value.

In order to determine I, values of θ and V were taken from the Malayan Meteorological Summary (1952) and other data provided by the Malayan Meteorological Service for the extreme months of January and June. Using $\theta_0 = 26{\cdot}6$°C, the diurnal variation of the function $V^{\frac{1}{2}}(\theta - \theta_0)$ was calculated for Singapore (lat. 1° 17′) and this is shown in Fig. 1. The area under the graph gives $I = -0{\cdot}41 \times 10^6$ in January and $+2{\cdot}1 \times 10^6$ cgs in June. For the greater part of the year it would appear that the expectation would be favourable, the duration of melting in the case under consideration being at least a week, although in extreme weather this might be reduced to a day or two. Tests show that the behaviour of diphenyl ether in the Singapore climate is not less favourable than this prediction. Data for Penang (5° 17′) and Kuantan (3° 40′) give similar values of I, and imply the suitability of this reference point for low latitudes in general.

4. Design

While diphenyl ether is, generally speaking, a very convenient substance for the present purpose in that it is chemically very stable, only slightly soluble in water, readily available in a pure state, fairly inoffensive, and only slightly inflammable, on the other hand, temperature

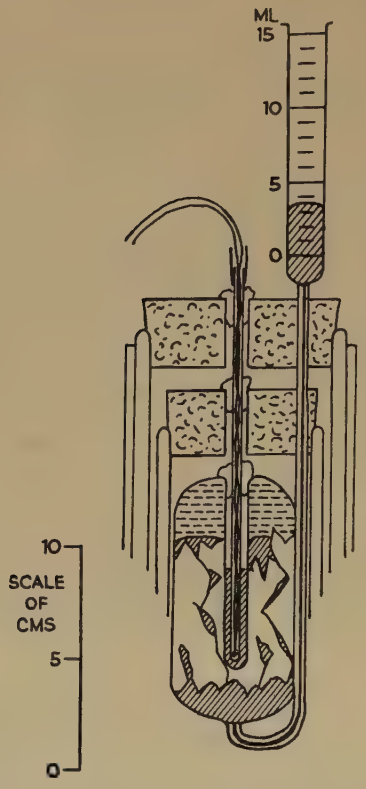

Figure 2. Diphenyl-ether ' calorimeter ' for the self-maintenance of the melting point as a thermocouple reference temperature.

uniformity after heating is attained only slowly, and the liquid phase is subject to severe supercooling. In addition diphenyl ether is not odourless, and is a powerful solvent. For these reasons it is undesirable to use a large quantity of diphenyl ether, or to permit rapid heat exchanges, to risk complete melting or to use it in an open vessel. It was also considered necessary to provide an index of the amount of solid remaining, as can conveniently be done by means of the volume change on melting.

A modified 135 cm^3 Bunsen ice calorimeter (Fig. 2) was filled with diphenyl ether which then, in order to remove dissolved air, was subjected to approximately 10 cycles of slow freezing and rapid melting under a low pressure. About 20 per cent of the diphenyl ether was replaced by mercury, the container was insulated with double, well-corked, vacuum flasks and left in a room at 25°C.

The expansion on melting, and the value of q were determined, the air velocity (5-10 cm/sec) being measured with a velometer, and the air temperature recorded.

5. Tests

With the diphenyl ether completely frozen, the assembly was placed in a fairly shady position between a tree and a building, at a height of 1·5 m above short grass, and provided with a large sheet of aluminium, bent into a hemi-cylinder with its axis along the meridian, as a radiation shield. Additional shields were later provided and appeared to be ineffective.

The behaviour of the diphenyl ether during one test which included a spell of especially hot weather is indicated in Fig. 3. It will be seen that 87 per cent melted in 18 days, and that even under the most unfavourable conditions the duration of melting would be about a week.

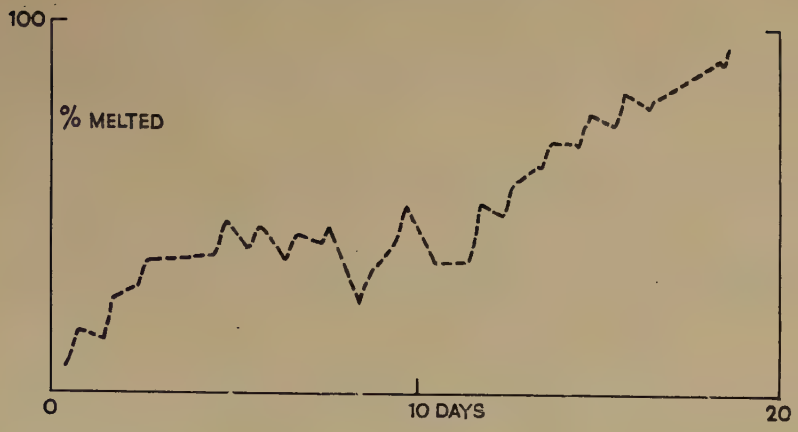

Figure 3. Test of the duration of melting of diphenyl ether in the open air at Singapore.

During the test the reading of a thermometer inserted in the well was taken. The mean temperature registered was 26·60°C with a standard deviation of 0·075°C. It was found that the maximum temperature reading occurred nearly 2-hr after the air-temperature maximum.

Acknowledgment

Thanks are due to Mr. Yang Shan Chiew for practical assistance.

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NOTICE

551.515.8 : 551.558.1 : 551.558.21 : 551.551

The sixth O.S.T.I.V. Congress – A report on the meteorological discussions

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The Sixth Scientific Congress of the Organisation Scientifique et Technique Internationale du Vol à Voile was held at St. Yan, France, from 6 to 13 July 1956.

Jet streams and squall lines

The first meteorological session, on jet streams and squall lines, opened with a talk by Professor W. Georgii on some general associations between the two phenomena observed in Argentina, West Africa and the United States. Following this broad commentary, Professor P. Raethjen used observations of turbulence and non-geostrophic winds in a particular jetstream over the United States to support his view that stream elements leaving the friction layer are accelerated during their ascent so as to form a super-geostrophic core.

Discussion of these two papers was brief and the delegates were soon intrigued by Dr. M. Ligda's film of radar observations of mature prefrontal squall lines in the midwestern United States. His accompanying paper described how these radar observations had, in recent years, shown a complex but organized internal structure of squall lines which cannot be deduced from normal weather observations. One of the most striking features of the study was the frequency with which phenomena interpreted as horizontal lightning had been observed and photographed on the radar screen itself. It appears that this lightning usually occurs between a firm, well-marked echo along the squall line and a rather ragged, broader belt of echoes to the rear. Some surprise was voiced when the length of horizontal flashes was declared to have a mean of 40 mi and a maximum of 100 mi, but the film showed at least one long and distinct ' stroke ' besides many other flashes which appeared to be reasonably interpreted by the radar observers as horizontal lightning.

Synoptic characteristics of squall lines over the midwestern United States were described by Dr. M. Tepper who reviewed their nature according to extremely detailed sets of observations from a close network of temporary stations organized by the United States Weather Bureau. He also reviewed the theory of steepening atmospheric gravity waves and showed how several synoptic patterns associated with the generation of severe storms had led to the dynamical production of such waves.

This session was to be concluded by Dr. M. Cadez speaking on the stagnation of cold fronts in the forenoon. But unfortunately Dr. Cadez was unable to be present and there was insufficient time for more than a very brief enunciation of his work. However, the paper was available and described at some length the study of cold fronts over Yugoslavia in 1951 and 1952. During the summer, outbreaks of cold air from the NW and N appeared to stagnate in the forenoon and begin to penetrate, often with considerable speed, in the afternoon. This phenomenon was explained by the heating of cold air over warm ground which tends to steepen the slope of the boundary, thereby effecting a stagnation of the front. Subsequent acceleration was ascribed to a sudden fall of pressure associated with high-level divergence ahead of the front.

Convection

The first of two sessions on thermal convection was opened by Dr. R. S. Scorer with a film showing the behaviour of visible liquid bubbles sinking through water. Parallels were drawn to the motion of isolated convection clouds and thermals in the real atmosphere. Dr. Scorer was wisely cautious in making these comparisons, but the film almost spoke for itself and it will not be surprising if it stimulates in the near future a few new ideas on the formulation of convection models and turbulent mixing.

Turning from isolated thermals to general convection in shallow layers, Dr. H. von Tippelskirch described his experiments on cellular convection. He has observed that, in liquid sulphur, the direction of flow in cellular convection depends upon the sign of the viscosity gradient as a

function of temperature. It was found that a tobacco aerosol behaves in a similar way including the transitional stage when both fluid and gas type of cells exist, the prevailing cell type depending mainly upon the temperature. Visual similarity between the experiment and shallow altocumulus and cirrocumulus shows some support for a theoretical relationship between the model and the real atmosphere.

Leaving laboratory experiments for field work in the atmosphere itself Dr. J. P. Kuettner then presented Dr. R. M. Cunningham's paper on flight observations of the fine structure of cumulus clouds. Selected cumulus clouds were penetrated by specially instrumented B-29 aircraft equipped with a microwave refractometer and a fast-response vortex thermometer. A resolution of 1/50 to 1/100 sec or about 3 ft in space was achieved. Typical patterns of humidity, liquid-water content and temperature were discovered which indicate the existence of a cold shell inside the cloud where liquid water is found in an unsaturated environment. All the flights were carried out over Arizona through cumuli with bases at about 14,000 ft.

Opening the second session on convection Professor H. Koschmieder illustrated his talk on the life cycle of cumulus clouds with several sets of serial photographs and sketches. He was followed by Dr. Ruppersberg with an account of wind-vane research at the German Gliding Research Institute. The behaviour of a wind vane was studied for the purpose of measuring the vertical wind components at radio towers. Wind-tunnel investigations showed three types of reaction: a damped oscillation, an asymptotic approach to a value of small reliability, and a large-amplitude oscillation due to self-excitation. The results were in satisfactory agreement with theoretical conclusions and an instrument profile was developed which avoids self-excitation, zero shifts and undue time lag.

The session was concluded with a return to laboratory work, described by Professor M. J. Bricard who outlined his research on direct microphotography of water droplets in the air.

Atmospheric waves

The first paper on waves was by Dr. R. R. Long whose earlier theoretical work was described at the previous O.S.T.I.V. Congress. This work involved a first integral of the primitive equations of motion of a stratified fluid with vertical shear. The resulting differential equation was reduced to a simple linear form in the case of a constant density gradient and a very small increase of velocity with height. In his latest paper, presented on this occasion by Dr. R. S. Scorer, it was shown that the differential equation is exactly linear for an infinite number of models, many having a shear and density gradient of geophysical interest.

After a brief discussion and short film of Dr. Long's experiments with waves in water tanks attention was turned to the theory of rotor flow in the lee of mountains. Dr. J. P. Kuettner maintained that a realistic theory must take into account the following observed facts: the excessive turbulence, the diurnal variation of intensity, with a peak in the afternoon, and the vertical extent of the roll cloud which may reach as high as 30,000 ft. Dr. Kuettner's explanation, involving a pressure jump preceded by convective heating in a two-layer atmosphere, could be made to account for the peculiarities of rotors in the lee of the Rockies, but Dr. Kuettner appreciated that it was not a unique or universal explanation of such phenomena.

Dr. M. G. Wurtele then spoke on waves in the lee of an isolated mountain. The theoretical model he uses is an isothermal atmosphere with no shear in the basic flow. If the perturbations are small, vertical velocities due to a circular mountain may be computed at distances from the mountain greater than one stationary wavelength. In vertical planes parallel to the basic flow the pattern is like that of the two-dimensional case studied by Lyra and Queney: the tilt of the wave crests decreases with increasing distance from the plane through the centre of the mountain. In horizontal planes the nodal lines of vertical velocity are hyperbolas with transverse axes along the wind direction; areas of upward motion are crescent-shaped, concave downwind, as often observed by pilots, and by Abe in his study of airflow over Fuji Yama.

Similar characteristics were derived by Dr. Scorer who has applied his lee-wave theory to the flow past an isolated hill of circular or oval shape. The theory presented by Dr. Scorer was restricted to a two-layer atmosphere but there is no theoretical objection to its extension to multi-layer models. The practical difficulty would, of course, be in the labour of computation.

Reverting to lee waves from the Rockies, Dr. Keuttner then read a paper by Dr. H. Klieforth on the structure of the mountain wave as measured in the Sierra Wave project. The first quantitative results of this project have now been prepared. Streamline cross-sections have been derived from optical and electronic tracking of instrumented sailplanes and are available for reference.

The second session on waves began with a film and commentary by Dr. J. Förchgott showing the development and periodic fluctuations of wave clouds in Czeckoslovakia. Three types of stationary wave were described. The most powerful wave – the lee wave – is generated by a low-level system of vortex pairs which are released periodically by the mountain ridge. This system, designated the ' active turbulence layer,' induces wave-like deformations and strong turbulence; it also determines the wind and pressure distribution on the lee side.

More field work on mountain airflow was then described by Dr. A. Gazzola and Professor H. Koschmieder whose observations, made in Italy and Germany respectively, added to the expanding list of wave characteristics which appear to be peculiar to the particular regions explored.

The topic of mountain airflow was also included in a joint session of the O.S.T.I.V. meteorologists and the pilots competing in the World Gliding Championships held concurrently at the nearby airfield. Presenting the latest news of the ' Mountain Wave Project,' Dr. Kuettner announced that both powered aircraft and sailplanes were now used to explore the high-level flow over the Sierra Nevada, the majority of glider flights exceeding 40,000 ft. The stratospheric flow showed unexpected characteristics : strong temperature and wind variations were recorded between 20,000 and 40,000 ft. Lee waves were restricted to the upper troposphere while the rotor phenomenon governed the lower troposphere.

Another mountain-airflow project was described by N. Gerbier and illustrated by a well-made film. This project concentrated on airflow across the Basses-Alpes during January 1952. A small team of meteorologists used gliders, a light aeroplane and a mobile radar unit to plot streamlines at levels up to about 30,000 ft. Frequent use was made of zero-lift balloons released by carrier balloons at predetermined levels. The results were most interesting and, together with further work which is planned, should form a useful set of data for comparison with any theoretical work which may be attempted.

Following this film the theory of lee-wave amplitude was discussed by C. E. Wallington. His theoretical work in collaboration with G. A. Corby shows the significant effects of mountain shape, wind and temperature structure on the amplitude of whatever lee waves are possible. As an illustration, the discussion extended to potential lee waves ahead of well-defined warm fronts. It was shown that well-marked waves are likely to be most pronounced in two, or possibly more, fairly distinct zones ahead of such fronts. Evidence for such a conclusion was obtained from a glider flight made in a wave at Camphill at the time of the previous O.S.T.I.V. Congress. The local topography and weather situation were carefully analysed and the streamlines computed for the time of flight, the computation being based on Scorer's technique.

Turbulence

The joint pilots' and meteorologists' session also included a film and talk by Dr. P. B. McCready on his research into the transport of freezing nuclei and on atmospheric turbulence. A sailplane equipped with a portable cold box was used as a probe for locating silver-iodide smoke released from ground generators during convective conditions. The results showed how important thermals are for carrying nuclei aloft, and the experiment demonstrated the value of slow light aircraft for such measurements of nuclei.

A sailplane was also used as a probe for the measurement of the frequency spectrum of atmospheric turbulence, because its aerodynamic characteristics simplify the measurement technique. Records of air velocity and vertical glider acceleration were taken by magnetic-tape recorder for subsequent frequency analysis. For a wide variety of turbulence situations it was found that the horizontal turbulence closely followed the theoretical ' energy $\propto$ (frequency)$^{5/3}$ law ' for all eddies smaller than 88 m, and the results implied reasonable isotropy of turbulence for eddies at least up to 50 m in wavelength. The measurement technique provides a quick experimental method for finding aircraft-response characteristics, as well as a method of turbulence measurement which is independent of the measuring vehicle.

General

Other contributors to the joint session included P. A. Wills speaking on unusual meteorological phenomena encountered in the Mackenzie Country of New Zealand, and Hanna Reisch on her experience in flying along an active squall line in Germany.

Naturally many of the discussions were carried on informally between the official sessions. This was in fact almost a necessity to those interested in any particular paper because the somewhat ambitious programme had not allowed sufficient time for discussion during sessions. In general the meetings were most valuable and stimulating. Thanks are due to C. Slikkerveer and Betsy Woodward for their organizing ability, to Dr. W. A. Eichenberger for his interpreting and Dr. J. P. Kuettner for his supervision, his interpreting and his infectious zest.

It is expected that those papers not already published will soon appear in the Swiss *Aero Revue*.

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Correspondence

551.510.42 : 551.551 : 551.558.1

The bent-over plume of hot gas

By P. J. Meade

In a recent issue of the *Quarterly Journal* (**82**, p. 165) C. H. B. Priestley discusses the problem of the ascent of a heated jet of air in the presence of a horizontal wind. The motion is considered in two parts – a first phase when the hot gas is diffused by the turbulence which it creates itself and a second when the dominant turbulence is that due to the environment. Application of the theory therefore depends upon a knowledge of two important parameters c and k, one for each phase, but even so comparison with published measurements of plume heights downwind from chimneys is encouraging.

The great difficulties inherent in the problem of the vertical jet subjected to an independent horizontal wind have been described by Sutton (1950) and an important objection to Priestley's paper seems to be the attempt to extend to an atmosphere in motion two separate theories derived for an atmosphere at rest and based on other limiting assumptions. One presumes that a semi-empirical extension of one theory alone would not have proved satisfactory.

In a paper (1955) submitted to the Meteorological Research Committee last year, I gave the following formula for the decay of vertical velocity in a stream of hot gas in a calm atmosphere :

$$w = w_0 \left(\frac{z + z_0}{z_0}\right)^{-\frac{2(a-b)}{3a-2b}p} \exp\left[-\frac{acz_0}{(3a-2b)(1-p)R_0}\left\{\left(\frac{z+z_0}{z_0}\right)^{1-p} - 1\right\}\right]$$

In this formula

z = height measured from chimney mouth

z_0 = distance of equivalent point source below chimney mouth

a, b are numbers depending on the horizontal variation of the axial values of wind velocity and temperature excess.

p is given by the increase of the radius of the jet with height, viz., $R = R_0\left(\frac{z+z_0}{z_0}\right)^p$

R_0 = initial value of the radius of the jet

c is a number which depends on the vertical shearing stress and which Priestley describes as the spreading coefficient.

In the appendix to this paper, by appeal to laboratory data and to Sutton's (1953) treatment of the diffusion of smoke clouds, it was suggested that the following numerical values could be inserted in the above equation :

$$\frac{a}{b} = \frac{4}{3} \text{ for fully turbulent motion}$$

$$p = \frac{7}{8}$$

$z_0/R_0 = 4$ approx.

The above expression is then reduced to :

$$w = w_0 \left(\frac{z + z_0}{z_0}\right)^{-7/24} \exp\left[-\frac{64}{3}c\left\{\left(\frac{z+z_0}{z_0}\right)^{1/8} - 1\right\}\right] \quad . \quad . \quad . \quad . \quad (1)$$

It was further suggested that this formula for velocity decay could be applied to the case of a jet ascending through an atmosphere in motion if it were assumed that c would increase as the horizontal ambient wind increased. With this assumption satisfactory agreement was obtained with plume-height data for chimney stacks at Oak Ridge, U.S.A. As it stands Eq. (1) is not directly applicable to the observational data used by Priestley in his Table 2 since a formula giving velocity decay with respect to time is required. However, by graphical integration, a comparison of similar type to that carried out by Priestley has been made and the results are shown in the following table.

TABLE 1. CALCULATED AND OBSERVED RISES OF PLUME AT VARIOUS DISTANCES DOWNWIND

Plant No.	w_0 (ft/sec)	Wind speed (ft/sec)		c	k	Rise of plume (ft) at stated distance (ft) downwind of stack						
						100	150	200	300	400	600	800
1	31	14	O*			60		99		156	185	
			P	1·0	0·085	54		91		135	154	
			F	0·25		70		102		148	183	
1	31	33	O			29		35		52	70	84
			P	1·5	0·20	24·5		39·5		58	66	69
			F	0·54		28		40		53	65	75
1	31	26·5	O			28		40		63	80	101
			P	1·35	0·16	29·5		48·5		72	82	86
			F	0·48		33		48		67	79	89
1	31	19	O			55		75		121	165	200
			P	1·15	0·115	42		67		101	114	120
			F	0·30		54		78		115	135	155
2	28	10	O			46	53	58	67			
			P	0·85	0·06	41	53	61	79			
			F	0·375		43	53	60	71			
2	28	13·3	O			36	38	41	47			
			P	0·95	0·08	32	42	49	62			
			F	0·42		35	42	48	57			
3	35·5	20·4	O			46		66	82	92	103	
			P	1·2	0·12	40		64	82	94	108	
			F	0·36		46		67	80	93	103	

* O – observed; P – Priestley's theory; F – formula of present note.

Summarizing the results given in this Table, use of the formula given in Eq. (1) above overestimates the height by an average of 1 per cent, while the average error irrespective of sign is 8 per cent. Corresponding figures for Priestley's theory are respectively 3 per cent (underestimate) and 13 per cent. It would appear therefore that the formula at (1) above with the single parameter c will give as good results as a more elaborate analysis involving two parameters as well as additional assumptions.

A question of considerable interest concerns the magnitude of the dimensionless quantity c. Priestley suggests that this should be about 0·1 in a calm atmosphere and, increasing as the square root of the wind speed, should be 1·0 when the wind speed is 14 ft/sec and 1·5 when the wind reaches 33 ft/sec. This range of variation is very large, and indeed the values of c for a horizontal wind appear surprisingly high because they imply an extremely rapid spreading of the jet since, according to Priestley's analysis, its radius is given by $R = cz$. The present writer's calculations suggest that for calm conditions c is rather greater than 0·1 and increases approximately linearly to 0·54 at a wind speed of 33 ft/sec, that is, a very much smaller range of variation. It also appears, as one would expect in any case, that the value of c for calm conditions depends on the characteristics of each chimney – rate of heat output, excess temperature at exit, etc.

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21 June 1956.

REPLY

By C. H. B. PRIESTLEY

The analysis given by Meade in *Meteorological Research Paper* No. 952 raises some significant points and it is of interest to see that it gives such good agreement with Bosanquet's measurements. The comparison in Table 1 however is not a fair one, since my c and k were fixed independently

of Bosanquet's measurements whereas Meade appears to have fitted c to the measurements themselves and even allowed a different range of values for each plant. I still feel that we must reserve judgment on which of the working theories gives the most accurate results until experimental data are available over a wider range.

One object behind my analysis was to promote, in conjunction with observations of the rise of plumes, measurements of their spreading and of atmospheric turbulence, for it seems clear that there is no unique relation between these two, and that each will strongly affect the plume's elevation. Any claim to provide even an approximate treatment dispensing with one of these parameters, or its equivalent, must therefore be open to suspicion on that very account. But Meade of course does not really reduce to a ' single parameter c ' for he requires to assume values for both p and z_0/R_0.

It is agreed that his analysis is the less elaborate when it comes to the computation of an actual example, but this is scarcely a serious consideration. Graphical or numerical integration is required in any event, and the problem is of sufficient importance to warrant the calculations being carried out once and for all to cover the relevant range of conditions.

My main objection to Meade's treatment is that it turns its back on certain issues which were raised in mine, and which were there at least set on the road towards their resolution. Eq. (1) gives w as a monotonic decreasing function of z, and so excludes any case in which vertical motion is generated by buoyancy, e.g., the plumes from smoke-pots used in frost protection. Neither this nor the preceding formula contain the plume temperature or Froude number, and they therefore imply that all plumes with the same w_0, R_0, z_0 will follow the same path, however hot they are. No indication is given of how to apply the formula when the environment is thermally stratified; presumably this might be done by adjusting p, but what are the guiding principles ? Finally it is seen from the complete solution in the M.R.P., Eq. (13), that the Eq. (3) of that paper is not satisfied : this equation here represents the condition that the temperature regime shall be in a steady state. The solution therefore does not apply to the steady state, nor indeed is it the solution of any properly formulated problem because only two basic equations have been given for the three independent variables R, θ, w.

In order to extend the range of experimental data my colleague, F. K. Ball, has made measurements of plumes from burning oil-pots under a variety of wind speeds. At the larger distances downwind the smoke was too disperse to measure accurately, but at shorter distances the heights agreed very well with my theory (first phase). Thus the tentative formula Eq. (5) of my paper stands up well, without change of the constant of proportionality, to a considerable change in scale and boundary conditions. We have little evidence at all about the second phase, and the limitations of my treatment in this regard are fairly obvious. A more rigorous treatment here would be most welcome.

To comment finally on two points from Meade's second paragraph, it is not clear why the extension to wind constitutes an important objection to my theory ; it appears to me less open to this objection than its predecessors. If Meade has arguments to the contrary I hope he will bring them forward, but in fact he has adopted the same philosophy as I have, i.e., the same functional form of z in the calm and windy cases, with c dependent on wind speed. The division into two phases was a practical step dictated not so much by shortcomings in the theory as by experience gained watching plumes. Others who have formed a similar habit will not need to be convinced that atmospheric turbulence is ineffective at the beginning but may come to dominate the mixing before all buoyant motion has been lost.

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551.558.21

Airflow over mountains

By P. A. Sheppard

At the Society's discussion of the above subject at its Aberystwyth meeting on 27 July 1956, I asked the question ' Under what conditions will an airstream rise over a mountain range ? ' The previous speakers had told us some of the happenings when an airstream does surmount a mountain range but little about whether it could do so. I received no very satisfactory answer to my question, for reasons of time or otherwise, and think it may be worth airing in these pages.

Shaw says somewhere that air has difficulty in rising over a mountain and generally prefers to go around the sides – no quantitative considerations are employed. It may, of course, do neither, in the sense that the presence of the mountain may produce a flow pattern which sidesteps the problem. To that extent my question may have no simple answer. Suppose, however, that the question has some meaning. Then the answer might be expected to be in some such terms as the air rises over the mountain when it has sufficient energy upwind of the mountain at its datum level to do so.

The equation of mechanical energy for a particle of air of unit mass in a frictionless gravitational atmosphere is

$$-\frac{dp}{\rho} = d\left(\tfrac{1}{2}q^2 + gz\right)$$

where $-dp/\rho$ (p is the pressure, ρ the density) is the work done on the particle by the pressure drop along an increment of path, and $d\left(\tfrac{1}{2}q^2 + gz\right)$ is the change in the sum of the kinetic energy ($\tfrac{1}{2}q^2$, where q is the velocity) and the potential energy (gz, where g is gravity and z height).

As an extreme oversimplification, regard the particle as moving in an environment such that the pressure on the particle at any level is the pressure at the same level in the environment. Then, denoting an environmental property by subscript e, $-dp = g\rho_e\, dz$, so that the energy equation becomes, using the gas law,

$$d\left(\tfrac{1}{2}q^2\right) = g\frac{T - T_e}{T_e}\,dz$$

where T is temperature. Assume the motion of the particle to be adiabatic ($T = T_0 - \Gamma z$ where T_0 is the temperature at the datum level) and to take place in an environmental lapse rate γ_e ($T_e = T_0 - \gamma_e z$). Then

$$q_0^2 - q^2 = 2g(\Gamma - \gamma_e)\int_0^z \frac{z\,dz}{T_0 - \gamma_e z}$$

where q_0 is the velocity at datum level, $z = 0$. Or,

$$q_0^2 - q^2 = \frac{2g(\Gamma - \gamma_e)}{\gamma_e^2}\left[(T_0 - \gamma_e z) - T_0 \log (T_0 - \gamma_e z)\right]_0^z \left(= \frac{g\Gamma z^2}{T_0} \text{ for } \gamma_e = 0\right)$$

From these formulae I find the following values of q_0 are required to enable the air just to surmount the level z ($q = 0$) in the given lapse rates. For unstable atmospheres, e.g., with cloudy air in a lapse rate greater than saturated adiabatic, energy would be released but the motion over the mountain would then presumably be complicated by convection.

TABLE 1. VALUES OF q_0 (M/S)

γ_e \ z (m)	250	500	750	1,000	1,500
0	4·6	9·1	13·6	18·2	27·4
0·5 Γ			11·0	13·4	
0·8 Γ		4·3	6·1	8·2	

It is interesting to see that the values of q_0 in the above table are not excessive in the light of commonly occurring winds. But they are certainly not correct since the equation of continuity has not been invoked and this requires that if the air does rise over the mountain, the mean velocity over the mountain top must be greater than above the datum level upwind. An opposite extreme oversimplification would presumably be to assume that the pressure field automatically adjusts itself to satisfy continuity in the manner of the potential flow solution. In that case energy considerations are of no consequence. I suppose the real solution lies somewhere in between these extremes, that is that velocity upwind of the mountain is of concern but not to the extent indicated by the above figures.

In view of the practical importance of this problem from many aspects, I hope that this letter may draw a response from those better qualified than the writer to express their views.

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2 August 1956.

55.584.32 : 551.524.4 : 551.524.36

The temperature profile above bare soil on clear nights

By J. V. LAKE

(Read 16 May 1956. See *Q.J.*, **82**, p. 187)

DR. J. L. MONTEITH : I should like to welcome Dr. Lake's paper because so far as I know these are the first published observations made in this country of a temperature lapse immediately above the ground on calm clear nights. I should not feel at all happy about using unshielded glass minimum thermometers under any conditions to measure air-temperature profiles, but the thermistor work has been very carefully executed and provides strong evidence that the effect is a real and not an instrumental one. The idea that these peculiar profiles might be produced by radiation error in the sensing instrument has been decisively disproved by Fleagle and Badgley in the report to which Dr. Lake refers. They were able to confirm the temperature lapse indicated with thermocouples by measuring the variation of the refractive index of the air, a completely independent method which can have no radiation error. These workers also showed that the temperature lapse could occur over different types of surface (e.g., grass, asphalt) and in different humidity conditions, and since the effect was most frequently observed when wind speeds were very low it was natural to suggest that an explanation might be found in radiative control of the profile. More recently, Fleagle has shown that radiative *heating* very near the ground and maximum radiative *cooling* at about 1 m can be predicted from a development of Bruinenberg's equation if certain assumptions are made about the shape of the water-vapour emissivity curve and the variation of temperature with height, so both pure theory and the experimental results which Dr. Robinson has described suggest that at night an atmosphere in radiative equilibrium would tend to produce a temperature profile with an elevated minimum.

Some observations of my own make me rather unhappy about this conclusion. Using shielded but non-aspirated thermocouples at six heights between 3·5 cm and 200 cm over a cricket field (with a fetch of about 200 yd) I have observed a temperature lapse replacing the normal inversion below 12·5 cm after periods of very rapid cooling. Cooling rates were of the order of 40°C/hr for periods of several minutes, a figure which is greater by an order of magnitude than rates which are predicted from radiation charts. I do not believe, therefore, that radiation can be the whole story but I should be glad to have comments on this.

DR. R. S. SCORER : It is difficult for those who are not experts in radiation to know exactly what it is that radiation theory tells us. For instance, are we to teach students that most of the heat lost is lost by the solid surface or not ? What suppositions about the composition of the atmosphere are necessary in order to explain these unexpected observations in terms of radiation alone ? Are such suppositions realistic ?

It is easy to understand that if cold air is above warmer ground there will be a ' viscosphere ' in which molecular conduction is paramount. Above this, either radiation or convection will be dominant, and in this case it appears to be radiation, but it is difficult to see how a temperature minimum so close to, but away from the surface is produced by radiation. I remain puzzled – could the radiation experts tell us what they can and cannot explain with realistic models ?

MR. R. W. GLOYNE : Apparently the sets of observations graphed in Fig. 4 and at least some of those in Fig. 5 are from readings of an array of minimum thermometers when the bulbs were ice-covered. If I understood Dr. Lake correctly, the single thermometer bead which was moved successively to different levels was also ice-covered at the time (Fig. 2, Tables 2 and 3).

These facts urge one to seek more information than was given in the paper or during the discussion on the interpretation of readings of ice- (or water-) covered thermometric elements in investigations such as this one. An ice- (or water-) covered ' bulb ' could be in association either with a gain of heat (by condensation) or a loss (by evaporation). An evaporative loss of heat would probably be a transient phenomenon in this investigation and in any case would be small unless there were sudden changes in humidity and a greater rate of ventilation than apparently occurred.

Presumably therefore the profiles in Fig. 4 and at least some in Fig. 5 and possibly in Fig. 2 are of dew (frost) point and give also by inference the vertical gradient of vapour pressure which obtain under ' strong radiative conditions.' As tests showed (p. 189) that ' screening ' the thermistor bead gave no measurable effect, we must also conclude that on the occasion discussed the air was saturated at all levels (if, indeed, the ' bead ' was ice-covered).

However, Dr. Lake suggests (p. 195) that there would still be a discontinuity in the *dry-bulb*

temperature profile in spite of the fact that readings from ice-covered bulbs were quoted. Doubtless this is true, for if we take the data of Table 2 *merely for illustration* and assume saturation at the 'surface' at 21°F then to achieve an isothermal profile (assuming adequate ventilation), the R.H. would have to drop to 89 per cent at 0·1 in., 82 per cent at 0·5 in., and to a minimum of 81 per cent. at 1·5 in.: saturation at 0·1 in. at $T = 18{\cdot}6$°F would imply a minimum R.H. of 91 per cent. at 2·5 in. for an isothermal layer from 0·1 in. to 54·0 in.

The mass of evidence discussed by Mr. Lake and (the additional evidence provided by Dr. Monteith) seems to leave no doubt of the validity of the phenomenon claimed, in certain readily-defined weather situations. Nevertheless it would be interesting to know whether Mr. Lake has profiles when the thermometer elements were certainly not ice- (or water-) covered: when the general level of temperature was much higher; what were the *actual* readings of the minimum thermometer (Fig. 4a) to compare with the roughly simultaneous thermistor readings given in Table 3; whether there was any mist or fog on the nights 20-21-22 March; and further, whether he has any thermistor readings in the early stages of the cooling process.

Mr. I. F. Long: Can Dr. Lake say to what extent the profiles are likely to be affected by large CO_2 concentrations at ground level? There is evidence that even over unmanured loam, on still nights, the CO_2 concentration in the bottom few centimetres may be ten or more times the normal value.

Dr. R. C. Sutcliffe: Although it is possible that the curious temperature profiles can yet be explained away as instrumental or advective effects the accumulation of evidence is such that the physical reality is almost certain and one looks for a thorough theoretical examination. The problem is of course one of slow change and near balance with radiative and diffusive processes, both important. One would like to see differential equations set up in order to discuss whether values can be given to the parameters to permit the observed profiles to occur. One might in this way arrive at a hypothesis on the physical quantities which could be checked by other experiments: it would seem to be an entirely approachable problem in this way.

Mr. J. V. Lake (*in reply*): The main feature of this discussion seems to have been doubt concerning the adequacy of a mechanism based on radiation exchange to account for the observed profiles. However, as I mentioned, Möller has made quantitative estimates of the rates of cooling which could occur if radiation were the only mode of heat transfer and these are more than adequate to account for the observed profiles. Möller insists that in all the cases he investigated radiative processes were swamped by convective processes, but Falckenberg, whose theory of 'radiative pseudo-conduction' is well discussed by Geiger in his book on The Climate Near the Ground, maintained that the reverse was often true. Thus the difficulty seems to lie in assessing quantitatively the part played by convection and molecular diffusion.

I am glad to find that Dr. Monteith has also observed profiles of this type. The key to an explanation of the rapid cooling he observed may lie in the fact that it only persisted for periods of several minutes. Best has observed large horizontal temperature differences over an apparently uniform level site on clear nights and a rapid change of temperature at a point could presumably occur if horizontal laminar air movement took place. This might not affect the overall cooling rate measured over a longer period.

In reply to Dr. Scorer, my results seem to support the view that the solid surface loses heat by radiation and cools at a rate predictable from Brunt's formulae, but that the situation in the air above depends markedly upon the prevailing conditions. Möller and Fleagle both indicate that realistic suppositions can be made about the water-vapour content of the air near the ground to explain the formation of profiles of the kind I have described, but until the difficulty is solved of deciding quantitatively what processes predominate under given conditions, the possibility of explanations with realistic models seems remote.

I do not know if the thermistor bead was coated with ice during the observations, but this was likely since other objects, including the thermometer bulbs, were so coated. I welcome Mr. Gloyne's clarification of the problem of ice on the bulbs, as I have no profiles for which I can guarantee absence of water or ice on the sensing elements. I did not record the actual readings of the minimum thermometers during the thermistor observations, largely because of the length of time this would have taken, nor have I any thermistor readings to correspond with Fig. 5. The nights of 20-21-22 March were entirely clear and there was no mist or fog.

In reply to Mr. Long, the main CO_2 absorption band at 15 μ, although intense, is very narrow and unlikely to account for more than 20 per cent of the energy exchange. An attempt to measure profiles of CO_2 concentrations and water-vapour pressures under these conditions, coupled perhaps with the radiative fluxes at different levels, would be very valuable and would no doubt ease the mathematical approach to the problem.

55.524.2 (42) : 551.524.36 : 55.588.7

Local temperature variations in the Reading area

By M. PARRY

(Read 16 May 1956. See *Q.J.*, **82**, p. 45)

MR. R. G. VERYARD : I would like to say how very useful are the studies of urban climate such as those carried out by Parry for Reading, Balchin and Pye for Bath and Manley for Manchester. In the Meteorological Office we are often called upon to give information and advice in respect of heating and ventilating requirements, etc., but most of the data at our disposal have been obtained from airfields. A detailed climatological survey of the Greater London area would be most valuable.

PROF. GORDON MANLEY : Mr. Parry has given us a very agreeable straightforward account of a large body of observational work. We need these studies of urban climate, even though they still beg about as many questions as they answer. For example, are all these sites round Reading well drained ? Kings Meadows, for example, might give very different minima if they were recorded nearer the river, as he has said; I think Mr. Richardson at Alston has shown that even a very small river can have an unexpectedly large effect. But one would further expect the differences to vary with the character of the season, wet or dry. The fact that this station averages 3°F colder on quiet clear nights and 1·1°F colder on all nights suggests that upwards of one-third of the nights fall broadly into the former category; I remember comparing the extremes at Yeovilton and Shaftesbury and reaching a similar conclusion, roughly two-fifths, in support of findings from Ushaw and Houghall. I do not know how far the station 9 (Forbury Gardens) is sheltered, but it is noteworthy that minima there appear to differ from Kings Meadow by very similar amounts throughout the year; at Manchester the differences, Oldham Road – Barton, were larger in summer. This leads to the view that the size and character of the enclosed gardens or yards where records are kept is probably more significant than the size of the built-up area. That it requires a stronger wind to eliminate the urban effect of larger cities is indeed interesting and I hope Mr. Parry will be able to expand this.

Unfortunately it is not easy to decide what happens as a consequence of the vast area of London because of the lack of homogeneity among the numerous available records. I took out some comparisons between Old Street and Regent's Park which suggest that on the average Regent's Park used to be about 5°F colder on clear nights; but one cannot readily compare with a more rural exposure on account of the baffling properties of the old north-wall screen at Kew, in which the minima may be up to 6°F above those in a Stevenson screen on the lawn.

I notice Mr. Parry had very little snow; a few nights with a deep snow cover would have sharpened up the differences. Studies of this kind are valuable as we are again led to ask what is a satisfactory exposure; which of all his Reading stations is the fairest representative of Berkshire, and what regard are we to pay to places like Buddon Ness, Newport (Salop), or Dartington near Totnes, in working out averages representative of a wide area ? The thermographs used were presumably bimetallic and subject to lag; I should be glad to know how far Mr. Parry was able to diminish this.

MR. C. I. JACKSON : Referring to Mr. Veryard's remark about the inadequacy of data for the London area, I understand that Mr. Chandler, of Birkbeck College, London, is attempting something similar to Mr. Parry's work. It is basically a study of the Lea Valley from the outskirts of London into the centre, and as well as using a network of fixed observing stations he hopes to obtain individual temperature profiles across the area by means of a car fitted with thermocouples.

DR. R. C. SUTCLIFFE : I agree with Mr. Veryard that local studies of this sort are of real practical value. It is much better to attack these problems of local temperature variations by controlled experiments with carefully-sited instruments and centrally-supervised observations than to try to estimate the variabilities by examining ordinary climatological data from such independent stations as happen to exist. Ideally one would like to see the whole country surveyed in this way so that forecasters and climatologists could give detailed advice on any locality; but failing this, a number of local studies would build up a very useful body of evidence to meet inquiries for agriculture, housing and other interests. I am sure that Mr. Parry's results will be immediately useful in this way.

Mr. R. E. Lacy (*communicated*) : It is at first sight surprising that maximum temperatures in the middle of the town on hot summer days are so little above those in the open country, for a town always seems much hotter. This is no doubt due to two factors – firstly, because the wind speeds in the town are lower and there is less convective and evaporative cooling of the body than in the country, and secondly because of the large amount of radiation emitted and reflected by the building surfaces. Under the intense solar radiation the walls and street surfaces will be considerably warmer than the air, probably 20°F or 30°F warmer, and so will emit much low-temperature radiation. In addition a good deal of short-wave radiation will be directly reflected from the surfaces. In consequence the mean radiant temperature of the zone in which people move will be several degrees above air-temperature, especially where there are fairly high buildings and the view of the relatively cool sky is limited.

In the open country conditions are very different. There is good exposure to any wind that is blowing, and the radiant temperature of vegetation will probably not be greatly above air temperature (although there is singularly little information on the effective radiant temperature of vegetation apart from some data for short grass given by Robinson (1950)). In open environments there is free exposure to the sky, the radiant temperature of which may be about 40°F below air temperature. In consequence, the net radiation gain of the body is much less in the country than in the town, and conditions are correspondingly more comfortable.

It may be said therefore, that, useful as the observations in this paper are, it will be necessary to take other measurements in addition to those of air temperature and wind speed in order to assess the relative degrees of comfort in town and country on warm days (or, indeed, on any day). What may be termed the ' radiation-climate ' is of great importance and, in addition, at the highest summer temperatures the wet-bulb temperature will also be significant.

As far as winter conditions are concerned, heating engineers are interested mainly in the accumulated temperatures below a given level (' degree-days ') and in the frequency of occurrence of low temperatures. The degree-day totals, usually referred to a base temperature of 60°F in this country, are used for estimating the total fuel requirements of a building during the whole heating season, or part of a season, and for comparing the fuel used from one season to another. Mr. Parry has already (1954) dealt with the variations in mean degree-day totals from place-to-place in the Reading area. The frequency data are required so that a suitable design-temperature may be chosen as a basis for designing heating-systems for buildings. Until recently this design-temperature has been selected in a rather arbitrary way, but a Ministry of Works publication (1955) gives the results of a study made by a committee headed by Sir David Brunt. It is recommended that the basic design-temperature should be selected on the basis of the frequency of occurrence of days with mean temperatures at or below a certain level. Daily mean temperatures were chosen rather than minimum temperatures because buildings respond quite slowly to changes in outside air-temperature, the lag being of the order of a day for most buildings. The design temperature chosen therefore depends upon the thermal capacity of the building concerned.

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Parry, M.	1954	' Local degree-day variations in the Reading area,' *Met. Mag.*, **83**, p. 307.
Robinson, G. D.	1950	' Notes on the measurement and estimation of atmospheric radiation,' *Quart. J. R. Met. Soc.*, **76**, p. 37.

Mr. M. Parry (*in reply*) : As Professor Manley points out, the Reading temperature data suggest that the character of the immediate site of a station is more significant than its surroundings. It should be possible to obtain convincing ' urban ' temperatures from a screen in the heart of the countryside provided it were sited in a small enough garden surrounded by walls or buildings. On the other hand, a station in an open park in the middle of a town may behave more or less as a rural station. Other things (like elevation) being equal, the minima at a given station on clear nights must depend largely on the amount of unobstructed sky available for outward radiation (though in industrial towns the smoke pall would invalidate this). Certainly, more work needs to be done on the question of what constitutes a representative site. In Reading, the Forbury Gardens station (with several trees and buildings subtending angles of over 40° at screen level) probably gives something near real urban temperatures.

Regarding Professor Manley's enquiry, the thermographs were bimetallic : their readings were regularly checked against the standard thermometers housed with them in the screens. The

thermograph records have so far been used to illustrate diurnal variations, etc., rather than for obtaining spot-temperature values.

I am glad to learn from Mr. Jackson that an investigation of part of the London area is being planned and that car traverses will be used. These have the great advantage of providing near-representative temperatures from the streets of the town, which ' properly ' exposed stations cannot do. On the other hand, fixed stations give fuller opportunities for the analysis of records.

I am grateful to Mr. Lacy for pointing out the significance of the ' radiation-climate ' in these town-and-country comparisons. In some large industrial towns the recorded maxima are often lower than those in the adjacent open country, owing to the reduction of solar radiation by smoke, but the town still seems warmer, no doubt because of the factors Mr. Lacy mentions.

551.501.81

On the space and time distribution of showers in a tropical region

By CHRISTINE M. SOANE and V. G. MILES

(Read 16 May 1956. Presented by Dr. A. G. Forsdyke. See *Q.J.*, **81**, p. 440)

Mr. A. J. Whiten : Showers, as I understand them, can be divided into three classes : (a) those of orographic origin, where little or no movement is to be expected; (b) convection showers, such as might arise here during the type of summer we hope to have this year, where just such movement and dissipation as is described would probably occur, and (c) showers associated with active and fast-moving cold fronts, where more lengthy persistence, with considerable movement could be sought. Are we to assume that the absence of this type of persistence and movement is due to the absence of such cold fronts in tropical zones ?

Mr. J. S. Sawyer : I welcome papers such as the present one which describe the experience of meteorologists in one part of the world for the benefit of their colleagues elsewhere, without awaiting full explanation of the phenomena concerned.

One is led to speculate on the reason for shower-prone areas or ' progressions.' In northern India, and elsewhere in the tropics, the important role of the downdraught of rain-cooled air from one storm in acting as a trigger for new storms is recognized. The spreading pool of cool air, canalised by local topography, can therefore lead to the movement of a system of showers or a ' progression ' at its edge. However, there are other possible explanations; for example the upper troposphere in an area where storms have occurred becomes cooler and moister by the evaporation of the cloud – an area where showers have developed is thus favourable for more showers and the system would move with the motion of the cooler, moister air in the wind aloft.

Dr. R. S. Scorer : Correlation of the direction of propagation of shower areas with the upper wind is not expected to be complete. If the downdraughts produced by the showers are the cause of the propagation, and this seems very likely, then the propagation would be in the direction of the wind shear (upper wind minus lower wind) relative to the motion of the air at or a bit above cloud level. This assumes that the ground is level, but topography can cause the downdraughts to be directed down the slope and this complicates matters. The main effect of topography alone would be to start showers over high ground in the morning and move them to a position over the valleys in the late evening. With these considerations in mind it is to be expected that order can be seen in the movements of the shower areas.

Dr. R. C. Sutcliffe : Weather radar has been used for research purposes for more than a decade and its use in operational meteorological services is increasing and may have a very important place in the future. It introduces a new phase in synoptic meteorology with detail in time and space on a new scale and it is important to study the new evidence on a research basis in order to discover any regularities of behaviour which may have prediction value. The present paper is a welcome contribution to this important practical problem as well as a stimulus to more basic research on convective systems in the tropics.

Mrs. C. M. Soane (*in reply, communicated*) : The clear distinction between the types of shower described by Mr. Whiten appears to us to go no further than the text-book. In practice how can they be differentiated ? The showers described here rarely occur only over high ground on any one day. We find no difference between the movement of individual showers

occurring over high ground and the movement of those elsewhere. On the other hand a well organized progression looks to an observer very similar to a cold front but we cannot detect any change in air temperature across it other than that due to the rain; true cold fronts are practically unknown here.

In answer to Mr. Sawyer, we agree that the ' cold pool ' theory is a possibility and have made attempts to track cold-pool movements to see if they are extensive enough and fast enough to be a feasible explanation. Unfortunately this investigation needs a denser network of stations than we can manage. It is my impression, though, that on many occasions the new outbreaks are too distant and too premature to be accounted for by this explanation.

In regard to the cold pool aloft, Mr. N. P. Sellick has found that statistically the upper atmosphere to 200 mb is distinctly warmer here in months with excess rainfall and cooler in months when rainfall is below average, which suggests that convection actually raises the upper temperatures.

Replying to Dr. Scorer, I agree that some systematic attempt should be made to get enough wind information for an investigation of wind shear and progression direction.

The remark of Dr. Scorer's about progressions moving over valleys in the late evening seems rather misleading. The evidence of later work has shown that in 159 cases, the initial outbreaks of shower activity occurred twice as often in quadrants of higher ground (NW and SE) as in the other quadrants. This figure may be significant but it still leaves one-third of the outbreaks unexplained. Thereafter we can only say that the progressions usually move; thus in two-thirds of the cases they necessarily move downhill but there is certainly no specific evidence that the progressions are usually over the valleys in the late evening : they may equally well be over another region of high ground.

We welcome Dr. Sutcliffe's remarks and would like to emphasize that showers in this area had been generally considered a random and disorganized weather feature despite the fact that they had been extensively studied because they are very important. Now radar has shown that organization is the rule rather than the exception and its employment is opening the way to further useful research and practical applications.

551.513.1 : 551.509.33 : 681.14

The general circulation of the atmosphere : a numerical experiment

By N. A. PHILLIPS

(See *Q.J.*, **82**, p. 123)

Dr. R. C. Sutcliffe (*President*) : Before asking Dr. Phillips to present his paper I have an important announcement to make. The adjudicators for the first award of the Sir Napier Shaw Memorial Prize (on the subject of the energetics of the general circulation) have now reported their findings and I am particularly happy to announce that the award goes to Dr. N. A. Phillips for the paper, already published in the *Quarterly Journal*, which is to be presented and discussed this evening. Dr. Phillips has for some time been at Princeton, at the Institute for Advanced Study, and will shortly be taking up a post in the meteorological department of the Massachusetts Institute of Technology, so further strengthening what is already one of the most important meteorological institutes of the world. It is our good fortune that Dr. Phillips has found it possible to cross the Atlantic to present his paper to us in person.

The President then congratulated Dr. Phillips on the award of the Napier Shaw Prize.

Prof. P. A. Sheppard : The sub-title of Dr. Phillips's paper is ' A numerical experiment.' It is on the conditions of the experiment – surely to be recognized as a magnificent experiment – that I wish to comment and learn a lesson.

By observation there is horizontal divergence of the flux of zonal momentum in the upper troposphere of low latitudes and convergence of the flux in the upper troposphere of middle latitudes. This divergence (convergence) must be balanced *in situ* either by convergence (divergence) of a vertical flux of zonal momentum or through meridional mean motion, polewards in low latitudes, creating zonal momentum, and equatorwards in middle latitudes, destroying zonal momentum. Corresponding conditions for balance are applicable in the lower troposphere

from which zonal momentum is exported by surface friction (including mountain drag) in the surface westerlies and into which momentum is fed by the same agency in the surface easterlies. Dr. Phillips makes the assumption that there is zero vertical flux of momentum at 500 mb, not merely by small-scale motions ($\tau_2 = 0$) but by the synoptic-scale disturbances which his model produces. He is therefore forced to meridional mean motions, direct in low latitudes, indirect in middle latitudes, to provide momentum balance. Moreover the strength of his indirect cell is approximately *twice* that of the direct cell. The direct cell is well substantiated now by the observations but there is still some query even as to the sense of any middle-latitude cell. Recent analyses of winds by Tucker and others suggest that the mid-latitude cell is indirect but about *half* the strength of the direct cell. Thus certain features of Dr. Phillips's results, and therefore of the conditions of the experiment, may perhaps require substantial modification. Against this, however, is to be set the appealing result that the calculated meridional mean motions give rise to the strong steepening of meridional temperature gradient in mid-latitudes.

I do not know an appropriate modification of the model leaving it mathematically tractable. Use of Richardson's $A = 0{\cdot}2\, l^{4/3}$ would give $A \simeq 10^6$ cm^2 sec^{-1} for $l \simeq 1$ km (for convective disturbances) and so $\tau_2 \simeq 1$ dyne cm^{-2} – a significant value. But convective disturbances are sporadic and the synoptic-scale disturbances might well be of greater importance. There is scope here for further empirical study.

And now, a question on strategy. I am curious to know why the author thought it significant to start his atmosphere from rest rather than from some more realistic ' initial ' condition (I occasionally talk to students about an atmosphere initially at rest but warn always that considerations cannot then be quantitative). Not merely has the atmosphere never passed through the ' at rest ' condition but assuming it to do so the equations describing its behaviour would be other than those used by the author. This part of the experiment, up to day 130, cannot then, I would suppose, have any ' quantitative ' value for meteorology.

Dr. E. T. Eady : I think Dr. Phillips has presented a really brilliant paper which deserves detailed study from many different aspects. I am in complete agreement with the point of view he has taken and can find no fault with his arguments, either in the paper or in the presentation. With regard to the criticism made by Prof. Sheppard and Dr. Sutcliffe I think Dr. Phillips's numerical experiment was well-designed. Numerical integrations of the kind Dr. Phillips has carried out give us a unique opportunity to study large-scale meteorology as an experimental science. By using a simple model and initial conditions which never occur in the real atmosphere he has been able to isolate, and study separately, certain fundamental properties of atmospheric motion – the kind of procedure adopted by all good experimenters. That these fundamental properties are significant is illustrated by the lifelike behaviour of the model in so many respects, both with regard to the structure of the disturbances which develop and to their transfer properties. An experiment which *merely* attempted to ape the real atmosphere would have been very poorly designed and very much less informative.

Mr. J. S. Sawyer : Fig. 13 of the paper suggests not only the formation of a jet stream in middle latitudes but also secondary jets to the north and south. Can the latter be regarded as similar to the sub-tropical jet stream, and can we deduce that it is established by a somewhat different mechanism from that responsible for the main jet stream associated with the long waves of the westerlies ?

Dr. Phillips drew attention to the pressure troughs in Fig. 8 which had features similar to fronts. Do the calculations in fact lead to pressure troughs which are more pronounced than the pressure ridges ?

Dr. G. D. Robinson : Dr. Phillips has developed his discussion of energy transformations in terms of interaction between the mean motion (i.e., the mean taken around a circle of latitude) and the disturbances. As he says, it is a method which has recently had much attention from meteorologists. I wonder if he would comment on the physical significance of the latitude mean zonal motions in the meteorological problem, particularly in the region of the westerlies. For example, there has been much comment on the direction of the transformation between the K.E. of the disturbances and the K.E. of the mean motion. It seems to me that this ' transformation ' is of arithmetical rather than physical significance, given the known facts concerning the size and distribution in latitude of the large-scale disturbances. If the K.E. of one or more of the disturbances is increased then the zonal mean wind must increase also. Fig. 3 of Dr. Phillips's paper is no doubt a valid *description* of the system ; does he also consider it offers a degree of *explanation* ?

Dr. R. C. Sutcliffe (*President*) : It is curious that equations which are very similar to the two-parameter models used in numerical forecasting (e.g., Sawyer-Bushby model used at

Dunstable) should lead to such slow developments. I think something must be done to make the computations more realistic in this respect, but apart from the time factor, has Dr. Phillips really done much more than show in this part of his paper that a baroclinic atmosphere gives rise to growing waves with cyclone and anticyclone development? This is a familiar result in dynamical meteorology and surely cannot go very far towards the explanation of the long-period mean of the general circulation. It would be interesting to know how much difference would be made by omitting heat input in this stage of the calculations and also omitting friction.

Referring to the energy transformations, the importance of the paper comes, I think, in dealing quantitatively with one of the oldest arguments in dynamical meteorology. In the case of small-scale turbulence it is probably correct to regard the eddies as a stage in the degradation of kinetic energy of the general current. But the calculations now presented indicate quantitatively that baroclinic developments are not of this character. The process is rather that the potential energy of the mean zonal circulation passes to potential energy of the disturbances; this is transformed to kinetic energy of disturbances which actually cause an increase, not a decrease, in the kinetic energy of mean zonal winds. The mean kinetic energy is balanced partly by degradation through small-scale turbulence and partly perhaps, as Dr. Phillips has suggested, by reverse meridional circulations. The significant point here is, I think, that owing to baroclinic developments the atmosphere tends towards stronger thermal gradients (fronts and jet streams) than are consistent with radiation balance and that the slow recovery processes tend to smoothing of the gradients and so to reverse circulation and a decrease in zonal kinetic energy. I think it is consistent with synoptic experience that in the absence of baroclinic developments in middle latitudes there is generally slow frontolysis and weakening of jet streams as the colder air mass warms up: the kinetic energy of disturbances and of zonal motion tend to decrease together but whether or not this means a reverse circulation in the vertical depends on whether or not the decrease can be accounted for by small-scale turbulence. I wonder whether Dr. Phillips would broadly agree with this description.

(*Added later*): May I suggest that the long wavelength and the slow rate of baroclinic development may be quite correct for an atmosphere with a gradual thermal gradient spread over a hemisphere and that this is what would in fact happen if the circulation over a symmetrical earth were to be built up from a state of rest as the experiment required. But this may have rather academic interest. The development of realistic cyclones may come at a later stage starting with the strong thermal gradients which the experiments showed must develop and the general circulation balance would then presumably depend on the organization of these smaller more vigorous instability systems which were not reached in this experiment. But Dr. Phillips's experiment suggests that the broad essential features of the general circulation may indeed be caused by the long waves in the westerlies; the shorter cyclonic disturbances may then be embroidery. This could be a very important result.

Dr. B. J. Mason: An important factor which Dr. Phillips has not taken into account in his experiment is the latent heat of condensation; for very good reasons he has considered only a dry atmosphere. But bearing in mind that one inch of rain releases enough heat to warm the whole atmospheric column by 8°C, it appears that extensive cloud formation which is associated with cyclones must contribute a good deal to the rate of energy conversion and therefore to the rate of development of these systems. This might, I think, have some relevance to Dr. Sutcliffe's criticism that natural disturbances develop more rapidly than predicted by Dr. Phillips's model.

It would appear very difficult to introduce the effects of condensation in a realistic yet manageable form – I should like to ask Dr. Phillips whether he has any ideas on this.

Mr. L. C. W. Bonacina (*communicated*): Can Dr. Phillips say whether there is any particular dynamical reason why the subtropical axis of high pressure with its anticyclonic cells between the easterly trade winds and the westerlies should be located in about latitude 30° N and S rather than in a somewhat higher latitude? If there is, it must be the critical factor restricting the width of the equatorial belt of cloud and rain with the inter-tropical front. This belt is, as we know, much disturbed by the irregular distribution of land and sea, but it moves north and south with the vertical sun and so would seem, in a sense, to be the mainspring actuating the extremely complicated mechanism of the general circulation of the atmosphere. If this be the correct interpretation of the equatorial belt, as the immediate thermal response to the vertical sun, all the other belts of wind and pressure must be adjusted dynamically to it.

Dr. R. P. Pearce (*communicated*): I would like to ask Dr. Phillips to comment further on the interpretation of the results near the upper and lower boundaries. It seems to me that the assumption $f = f_0 =$ constant makes it difficult to justify this regarding the computed flow in, say, the lower third of the patterns as *explaining* the flow in low latitudes, where the Coriolis term in

the equations of motion must be of the same order as, or less than, the acceleration term, even on the large scale he considers. I suggest that the real reason for the broad agreement in these regions lies in his choice of boundary conditions, namely $\phi' \equiv 0$, $\zeta' \equiv 0$. The former is realistic by symmetry across the equator. The latter, implying that no vorticity is created on the equator itself, which he describes as imposed ' somewhat arbitrarily,' is in fact also realistic since the Coriolis parameter is zero and pressure forces alone under these conditions create negligible vorticity. The true state of affairs is a gradual transition from approximately geostrophic flow in middle latitudes to ' Eulerian ' flow near the equator. The model used effectively approximates to this by taking the Coriolis parameter as a step function of latitude discontinuous at the equator, instead of as a cosine function. This may explain the rather strong Easterlies near the equator in the computed model.

The assumption $\zeta' \equiv 0$ at the boundary $y = +W$ is more difficult to justify. The vorticity equation at the poles demands $w = 0$ if $\zeta' = 0$ as seems to be borne out by Fig. 10. Can Dr. Phillips provide any justification for this assumption ?

Dr. N. A. Phillips (*in reply*) : Professor Sheppard's remarks concerning the strength of the observed indirect cell in the atmosphere are very interesting, and I would also agree that the indirect cell in the experiment is probably overestimated. The computations made at U. C. L. A. show that about 75 per cent of the observed convergence of eddy-momentum flux into middle latitudes occurs above the 500-mb level. In order to eliminate the indirect cell completely, one would then require a downward flux of momentum at the 500-mb level equal to about 75 per cent of the surface stress. Perhaps the stress is almost as large in the free atmosphere of middle latitudes, we just do not know. Our ignorance about this will certainly be a barrier to much further progress in either the study of the general circulation or the extension of present numerical methods of forecasting to longer periods. With regard to the importance of the large-scale vertical stresses, I would refer to Table 6 in the paper, where some rough calculations suggest that they may be important in determining the relative strength of polar and subtropical easterlies.

Dr. Eady's kind remarks answer quite well the question on the strategy of the experiment. If the initial conditions had been more realistic, it would have been more difficult to interpret the results, since one would never be sure that the forecast was not biased by the (realistic) initial flow patterns. The part of the experiment up to day 130 has indeed only pedagogic value, as Professor Sheppard suggests. For example, the equations used there do not allow of any steady-state solution in the absence of the diffusion terms.

Mr. Sawyer's question on the secondary jets is difficult to answer. As can be seen from Fig. 9 in the paper, they are associated with secondary maxima in the latitudinal temperature gradient caused by the bringing of relatively warm air to the north and cold air to the south, as if the poleward transport of sensible heat had overshot the mark, so to speak. Whether this is what causes the appearance of secondary jets in the atmosphere I cannot say, although it would seem to be an interesting possibility. With respect to the relative sharpness of troughs and ridges, the forecast equations do produce ridges in the ' stream function ' which are as sharp as the troughs. However, if one were to measure the pressure field by returning to the equations of motion (or by using the so-called balance equation) the pressure troughs would be accentuated and the pressure ridges flattened.

In answer to Dr. Robinson's comments, I would agree that Fig. 3 and the concept of the energy transformations are not by themselves an explanation of the system. Their principal value lies in describing which scales of motion are important in the generation of kinetic energy, and also how the largest-scale motion – the zonal mean motion itself – receives energy from the synoptic-scale disturbances to balance that which it loses to the very small-scale motions (which have been lumped together in the frictional terms). The ' explanation ' of this state of affairs is of course not simple, but perhaps could be best summarized by invoking the well-known tendency for rotation to reduce the horizontal dimensions of the dominant convective cells, so that the principal generation of kinetic energy takes place on a smaller scale than that of the zonal mean motion. Whether this results in a positive value of the transformation $\{K' \cdot \overline{K}\}$ will in general depend on other factors as well, for example, the dimensions of the system and its gravitational stability. In the atmosphere the situation seems to be that depicted in Fig. 3.

In reply to Dr. Sutcliffe, I would say that the experiment has shown more than one can infer from the linearized baroclinic stability studies, although not as much as I had hoped for, because of the truncation errors. For example, the formation of fronts and jet streams is not easy to predict from the linearized theories, and the maximum development of the disturbances is perhaps impossible to predict from those theories alone. I would certainly agree, however, with Dr.

Sutcliffe's description of the energy transformations. Concerning the question of gradual versus concentrated thermal gradients I would like to say this. First of all, I think it is not clear that the rate of *baroclinic* development was so much slower in the experiment than in reality. All that is certain is that the system developed for a longer time than do the more striking of the systems we see on the weather charts, and that it had larger dimensions. The more explosive developments found on synoptic charts may indeed be due to the more favourable effect of a concentrated thermal gradient, as Dr. Sutcliffe suggests, but may also be due to several other factors. The long forced disturbances in the atmosphere may have a considerable effect on the shorter free disturbances, both in their early and final stages, energy dispersion of the type discussed by Rossby being added to the ordinary baroclinic instability. Some recent work by Fjørtoft reported in *Tellus* is very suggestive of this. Certainly in the dish-pan experiments, where the forced disturbances are absent, it is common to find extremely steady-state disturbed flows. Finally, the release of latent heat should favour a more rapid development as well as a smaller dimension of the disturbance. I hope to make some further computations this winter, using a crude smoothing technique to reduce truncation errors, so that the experiment can be continued far beyond the point where concentrated thermal gradients exist, and thereby give some more information about the interesting point raised here by Dr. Sutcliffe.

Dr. Mason's remarks on the importance of latent heat are very pertinent. Some recent calculations I have made on the east coast storm of 24 November 1950 in the United States show, I believe, that the release of latent heat contributed significantly to the extreme deepening of that storm, although the ' dry ' equations still give a useful prediction of its formation. The fact that about one-third of the poleward energy transport occurs in the form of latent heat cannot be disregarded for long, either in a study of the general circulation or in shorter-range forecasts. Dr. J. Smagorinsky of the U.S. Weather Bureau is currently working on this problem, and has already obtained some encouraging results.

In reply to Mr. Bonacina, I can say that there is no brief simple dynamical reason for the location of the high pressure belt that I know of. However, I would interpret this numerical experiment (especially the discussion on page 152) as demonstrating how the transfer properties of the large-scale eddies of middle latitudes produce the observed surface wind distribution. I think I would also prefer to consider the intertropical convergence zone as the result of processes occurring over the earth as a whole – general heating in low latitudes and cooling in high latitudes – rather than as an immediate response to the vertical sun. The convergence zone does not move from tropic to tropic with the seasons, and this would also indicate to me that it cannot be considered as a mainspring, but rather as one of many springs, all of which are equally important for the general circulation.

Dr. Pearce's remarks on the lateral boundary conditions concern what is probably the weakest point in the mathematical formulation. My choice of these conditions was governed by several considerations. They should be simple, they must lead to meaningful energy and momentum integrals, and they must be conducive to reasonably ' quiet ' motions near the boundaries, since most of the activity in the real atmosphere is in middle latitudes (at the same time, however, they should not by themselves force any preconceived motions, *e.g.*, easterly winds in low latitudes, on the system). I did not attempt to justify them in as much detail as Dr. Pearce has outlined for the equatorial boundary because I felt that the equations and the geometry were both too crude to merit such treatment. I therefore cannot offer any more specific justification for the boundary condition at $y = + W$. Concerning the remarks about low latitudes, I can only refer to the reservations I made on p. 137 of the paper, although I do not believe that the broad agreement obtained at those latitudes is entirely fortuitous or completely due to the choice of boundary conditions.

Proceedings at meetings of the Society

Special General Meeting, 20 June 1956

A Special General Meeting of Fellows was held to elect the Officers and Council for the year 1956-57.

Fellows had been circulated on 10 April with the list of nominations by Council for Officers and Council for the year 1956-57. No other nominations having been since received, the persons so nominated were declared to be elected unopposed in accordance with By-law 51.

The Officers and Council for 1956-57 were therefore elected as follows :

PRESIDENT

REGINALD COCKCROFT SUTCLIFFE, O.B.E., Ph.D.

VICE-PRESIDENTS

Professor GORDON MANLEY, M.A., M.Sc.
SIR OLIVER GRAHAM SUTTON, C.B.E., D.Sc., F.R.S.
RICHARD GEORGE VERYARD, B.Sc.
THOMAS WILSON WORMELL, M.A., Ph.D.

VICE-PRESIDENT FOR CANADA

DONALD PATRICK MCINTYRE, Ph.D.

VICE-PRESIDENT FOR SCOTLAND

JAMES ROBERT ATKINSON, M.A.

TREASURER

JAMES STRACHAN FARQUHARSON, M.A., D.Sc.

SECRETARIES

PATRICK JOHN MEADE, O.B.E., B.Sc.
HOWARD LATIMER PENMAN, M.Sc., PH.D., F.INST.P.
GEORGE DAVID ROBINSON, PH.D., F.INST.P. (Editor)
ARTHUR GEORGE FORSDYKE, PH.D., D.I.C., A.R.C.S. (Librarian)

COUNCILLORS

CHARLES JAMES BOYDEN, B.A.
JAMES MARSTON CRADDOCK, M.A.
Instr. Capt. CHARLES ROY DARLINGTON, B.Sc., R.N.
THOMAS VIVIAN DAVIES, M.Sc.
THOMAS HERBERT ELLISON, M.A., PH.D.
RONALD WESTLAKE GLOYNE, B.Sc.
ROBERT FREDERICK JONES, B.A.
HUBERT HORACE LAMB, M.A.
BASIL JOHN MASON, D.Sc.
FRANK PASQUILL, D.Sc.
JOHN STANLEY SAWYER, M.A.
CHARLES EDWARD WALLINGTON, M.Sc.

Ordinary Meeting, 20 June 1956

The following candidates were declared to have been elected that day by the Council:

FELLOWS

CAMERON, Hugh, 315 Bloor Street West, Toronto, Ontario, Canada (*transfer from Associate Membership*).
GILLINGHAM, Angus Melvin, 315 Bloor Street West, Toronto, Ontario, Canada.
PROSSER, Peter, Amesbury Lodge, Hindhead, Surrey.

FOREIGN MEMBER

TANIGUCHI, George, 2015 Corinth Avenue, Los Angeles 25, California, U.S.A.

ASSOCIATE MEMBERS

ALLISON, A. E., 315 Bloor Street West, Toronto, Ontario, Canada.
EAST, T. W. R., Physics Department, McGill University, Montreal, Canada.
HARRISON, Gordon Harry William, 70 Davies Crescent, Kalamunda, Western Australia.
HOEPPNER, E. D., 315 Bloor Street West, Toronto, Ontario, Canada.
JARVIS, E. C., 315 Bloor Street West, Toronto, Ontario, Canada.
LOGAN, D. W., 315 Bloor Street West, Toronto, Ontario, Canada.
MACKENZIE, P. M., 315 Bloor Street West, Toronto, Ontario, Canada.
MANN, A. S., 315 Bloor Street West, Toronto, Ontario, Canada.
MURDEN, W. D., 315 Bloor Street West, Toronto, Ontario, Canada.
NURLIK, A., 315 Bloor Street West, Toronto, Ontario, Canada.
PEARCE, Robert Harwood, B.Sc., 70 Davies Crescent, Kalamunda, Western Australia.
ROGALSKY, J., 315 Bloor Street West, Toronto, Ontario, Canada.
SEBASTIAN, F. J., 315 Bloor Street West, Toronto, Ontario, Canada.

The President then announced that news had just been received of the adjudicators' decision to award the first Napier Shaw Memorial Prize to Dr. Norman A. Phillips, of The Institute for Advanced Study, Princeton, U.S.A., whose paper 'The general circulation of the atmosphere: a numerical experiment' (*Q.J.*, **82**, p. 123) had been agreed to be of a very high standard. By a happy coincidence, Dr. Phillips was present to read his paper to the meeting.

Dr. Phillips then presented his paper and a most interesting discussion followed.

Summer Visit, 11 July 1956

The Summer Visit of the Society was held on 11 July 1956 at the British Scientific Instruments Research Association, Elmstead Woods, Chislehurst, Kent, by kind invitation of the Director.

Some 30 Members and guests attended and were given the opportunity of inspecting most of the departments of the Association, including the Library and Information Service, which form an important part of the Association's work.

At the close of the Meeting a vote of thanks was moved by the Secretary, Dr. H. L. Penman, who expressed appreciation of the excellent arrangements made by the deputy Director, Dr. A. J. Maddock.

Two-Day Summer Meeting, Aberystwyth, 26-28 July 1956

The third two-day Summer Meeting arranged by the Society was held in honour of Sir David Brunt, Sec. R.S., whose seventieth birthday fell this year, and, by kind invitation of the Vice-Principal, Professor R. M. Davies, took place at the Pantycelyn Hall of Residence of the University College of Wales, Aberystwyth.

The meeting was formally opened by the President, Dr. R. C. Sutcliffe, O.B.E., at a Reception by the Vice-Principal of the University College of Wales, during which Sir Graham Sutton, F.R.S., paid a tribute to Sir David Brunt for his outstanding contributions to the advancement of the science of meteorology. Sir David replied with some interesting reminiscences.

The meeting consisted of two scientific symposia. Sir Graham Sutton was in the Chair at the first meeting, which was opened by the reading of papers by Mr. E. M. Job and Dr. M. J. Jones (Aberystwyth) on ' An ionization method of measuring air density,' with a display of apparatus, followed by discussion.

There was then a discussion on ' Topographical influences on airflow,' opened by Mr. G. A. Corby and Dr. R. S. Scorer.

Sir David Brunt took the Chair at the second meeting, which was opened by the reading of papers by Mr. G. M. Howe (Aberystwyth) on ' Observations on local and micro-temperature variations in West Wales ' and Mr. J. A. Taylor (Aberystwyth) on ' Air and soil temperature variations in relation to aspect on coastal slopes in Cardigan Bay.'

A discussion then took place on ' Orographic effects in relation to rainfall,' opened by Mr. J. S. Sawyer, Professor P. A. Sheppard, Mr. S. E. Ashmore and Mr. P. H. Walker.

A very enjoyable excursion by coach, attended by the President and some 60 Members and guests, was organized by Professor E. G. Bowen (Aberystwyth) to the reservoirs of the Claerwen and Elan valleys, where the party was met by Mr. C. A. Risbridger, B.Sc., M.I.C.E., City of Birmingham Water Engineer. The outward route traversed the old coach road from the Midlands into Central Wales and Professor Bowen added greatly to its interest by his commentary on the general physical and human geography of the area. At tea the thanks of the Society were expressed by the President, Dr. R. C. Sutcliffe.

On Saturday afternoon a visit was paid to the National Library of Wales, Aberystwyth, by kind invitation of the National Librarian, Dr. Thomas Parry.

Council wish to record their sincere gratitude to Professor E. G. Bowen, who was responsible for the local arrangements, and to the many others who helped to make the meeting a success scientifically and socially.

Reviews

Weather analysis and forecasting. Vol. I. *Motion and motion systems.* By Sverre Petterssen. New York (McGraw-Hill), 2nd Edition, 1956. Pp. xix, 428; 241 Figs., 20 Tables. $8.50 (64s.).

The appearance of this first part of the second edition of *Weather analysis and forecasting* is an important event in meteorological publishing. Amongst the limited number of systematic text-books on those parts of meteorology of most immediate interest to professional forecasters this work will have an assured position; it should be studied by every forecaster and by every university student interested in the subject.

Although described as a second edition the work is effectively a new book. I have not attempted to discover what is common to the 1940 edition, but it is a point of no importance as the book is planned quite differently and with the second volume, yet to appear, will run to something like twice the original length. *Motion and motion systems* is a novel title but broadly it deals with the problems arising in the preparation of prebaratic or prognostic charts, the large-scale circulations, their structure, dynamics and kinematics, leaving ' weather ' for the second volume. A complex science does not allow of a completely satisfactory breakdown but the two-volume arrangement is consistent with the forecaster's normal way of thinking and is surely as good as any.

It is a little unfair on an author that the great effort of preparing a basic text-book should need to be completely repeated so soon, but any text on large-scale circulations written before 1940 must be as dated as was, within a few years, any work on synoptic meteorology written before 1918 when frontology began. The direct three-dimensional aerology permitted by the introduction of the radio-sonde, together with the remarkable new advances in dynamical insight which quickly followed, have changed the outlook of the synoptic meteorologist and it is fortunate that we have in Dr. Petterssen someone who has followed and often led the advances in the hard school of practical synoptic forecasting to find himself now as a university professor with such practical experience, theoretical knowledge, opportunity and energy as rarely come together in one man.

I shall not list the chapters. They are concerned with kinematics and dynamics, with convergence and divergence, circulation and vorticity, long waves, baroclinic instability, cyclones and anticyclones, indeed all the concepts which are nowadays current in synoptic dynamical thinking. The standard of treatment is as consistent as one could hope. Simple vector analysis and partial differentials are used freely but with little elaboration of the algebra since, for the most part, general principles rather than the mathematical solution to specific problems are dealt with and discussed. Although the volume will be usefully studied by the advanced mathematical physicist interested in meteorological circulations it is not quite his book. The aim is clearly to take the meteorologist with a modest mathematical equipment as far as he can go towards understanding the processes quantitatively, and explanations of the physical meaning of the mathematical expressions will go far to ease the path of the student even if they sometimes rather irritate the advanced research scientist.

The last two chapters are on numerical forecasting (written by Dr. A. Eliassen) and on graphical integrations based on Dr. Fjörtoft's well-known work. These brief introductions to important new work call for a slightly higher standard of mathematical attainment but are very valuable as pointers to the future.

Dr. Petterssen's first edition became seriously inadequate within a decade. One wonders how this new and thoroughly up-to-date work will stand the stress of time; will this first part, on large-scale motions, find itself in the melting pot again within another ten or fifteen years ? It is indeed quite possible. Nowhere else in natural science is it required to predict the continuously changing three-dimensional flow patterns of a fluid in permanently unsteady motion. So long as the problem cannot be treated on a field basis by general hydrodynamical methods we must use specific tools and handle specific phenomena. The simple extrapolation of the behaviour of depressions and anticyclones was the first stage; the analysis, movements and modification of fronts and air masses came next; the present stage is a curious mixture, the supplementing of the old and well-tried methods with a number of kinematical and dynamical concepts and special rules suitable for certain parts of the flow pattern; one must think in terms of divergence, vorticity, deformation, abstractions of no practical interest in themselves to a forecaster concerned with winds and weather.

If hydrodynamical quantitative predictions have the success which is hoped for them it may very well be that these concepts will be swallowed up in the general equations, to emerge only in examination questions. Then from Dr. Petterssen's Chapter I on ' Basic equations ' we shall move direct to quantitative integrations; all the theory of movement and development, of the evolution of the patterns, will be implied and the theory and method of large-scale forecasting will be presented as a coherent deductive system instead of, as at present, a complex of ideas and procedures.

But there is still a long way to go and meanwhile Dr. Petterssen brings us up to date and provides a text which is much required.

The book is excellently produced; in a fairly careful reading I found nothing unacceptable and the few misprints I noted will not mislead anyone.

R.C.S.

Surveys in mechanics: *G. I. Taylor 70th Anniversary Volume.* Edited by G. K. Batchelor and R. M. Davies. Pp. vii, 475; Figs.; Tables. Cambridge University Press, 1956. 50s.

Sir Geoffrey Taylor came to meteorology in 1911 (as Schuster Reader at Cambridge) and immediately made his impress on the subject. But meteorology was hardly ever his only ' scientific ' love. This has been shared with fluid mechanics generally, with the structure of metals, the swimming of spermatozoa, and

the sailing of ships – as a sport and a study. None has failed to acquire the ' G.I.' impress. What a happy combination of seeing eye, flair for the significant experiment and mathematical dexterity his record betokens. G.I. is indeed a legend, a sport which comes but rarely.

Now, on his 70th birthday, and while still in full flight of imaginative enterprise, some of his friends and colleagues have thought to celebrate the occasion with a volume in homage, consisting of essays on subjects which Taylor at one time or another has himself adorned. There are ten of these, prefaced by a delightful short biographical note by Sir Richard Southwell. Every article is a survey of the present position in some branch of mechanics and all, as I can judge, have Taylorian character. Some have a highly individual stamp, true monographs, with original thought and results, and one senses the pleasure of their authors in writing them.

Of the ten essays one, by T. H. Ellison on ' Atmospheric turbulence,' is directly meteorological. Several of the others are on subjects which the meteorologist, in one or other of his activities, would do well to look into, so that a few words on these are appropriate. Thus we have Professor N. F. Mott on ' Dislocations in crystalline solids,' a subject now of interest to the cloud physicist in the study of ice-crystal growth. Mott's article is more concerned with the elastic properties of crystalline solids than with the role of dislocations in promoting crystal growth but the nature and transformations of dislocations are admirably expounded. Then there is a short essay by Professor H. B. Squire on ' Rotating fluids,' one part of which discusses work, pioneered by Long at Johns Hopkins, on wave motion induced by the movement of a symmetrical obstacle along the axis of rotating fluid. This may be of interest in relation to the study of lee waves in the atmosphere.

An essay of particular fascination is that by W. R. Lane and H. L. Green on ' The mechanics of drops and bubbles.' These authors describe techniques for the production of drops, their behaviour, including break-up, in falling through another medium, the impaction of drops on surfaces including the surfaces of larger drops, and the behaviour, including bursting, of bubbles. All these matters are grist to the mill of cloud microphysics and the physics of condensation nuclei, and the meteorologist, whether specialist or layman in this field, can be promised huge enjoyment from a reading of the essay. Taylor's name is much in evidence throughout.

Dr. F. Ursell reviews a long line of work on the generation of waves by wind and ends mainly with question marks. There is still no acceptable theory of wave growth or of the interaction of wind and water but the description of the phenomena in this field has gone forward rapidly in the post-war period so that one feels the subject is ripe for rapid theoretical development. The reaction on meteorology should then be quite significant.

Dr. G. K. Batchelor and Dr. A. A. Townsend expound in a scholarly fashion on turbulent diffusion, a study made very much their own in the post-war period. The subject and the article are difficult and useful results are slow to appear. Most notable perhaps is the stress laid by the authors on the importance of molecular properties (viscosity, conductivity) in any proper description of turbulent diffusion. The reviewer, as a meteorologist, felt a little depressed by such remarks as ' It is not possible to report any progress at all in efforts to deduce turbulent transfer rates in directions in which the mean velocity varies with position,' or again ' The interpretation of observations made on a single realisation of the turbulence field lies outside the scope of our survey . . . . There is some doubt whether (important integrals) converge for the *peculiar* case (reviewer's italics) of atmospheric turbulence, for which stationariness in time is not a clear-cut concept.' Meteorologists, however, should perhaps be stimulated rather than depressed by such remarks.

And now a word on the meteorology as such, by Dr. Ellison. It is a notably stimulating article and for this some faults may be readily excused. Thus it is not particularly well-balanced or well-informed and there are numerous minor errors. But it does indicate rather well the direction in which the study of atmospheric turbulence is heading and, read with discernment, should help others along fruitful paths. This is its vindication. The review is almost entirely concerned with empirical knowledge (what else, in view of the remarks quoted in the previous paragraph ?) although a new-comer to the subject might imagine, as others in the past have done, that there is quite a scaffolding of theory on which to hang the observations. Ellison has in this review provided some new scaffolding, rational empiricism I suppose it might be called, on which stability effects in the constant shear-stress zone and barotropic and baroclinic effects in the variable shear-stress zone should look more rational. His chapter on fluctuation studies is disappointing in content but no doubt a good deal of original work would be required to make much more of the observational data presently available.

This volume, which begins with a photograph of Sir Geoffrey Taylor in characteristic pose – or lack of it, and concludes with a delightful essay on the mechanics of sailing ships and yachts, has given the reviewer intense pleasure which he hopes many other meteorologists are going to share. It has the spirit of ' G.I.', that work is play, though often hard play. At 50s. it is uncommonly good book value for today, being beautifully produced and really priceless. So put it on the personal shelf if it can be managed and, if not, perhaps the libraries will acquire sufficient copies for borrowers to keep one for more than the usual term.

P.A.S.

Scientific Proceedings of the International Association of Meteorology, 10th General Assembly, Rome, September, 1954. Published for International Union of Geodesy and Geophysics by Butterworth Scientific Publications, London, 1956. Pp. xiii, 594; Figs.; Tables. 80s.

It is debatable whether the proceedings of a scientific congress, which is not confined to a single field, are appropriately published in a special volume. The reviewer would certainly prefer original contributions

made to such a congress to be published in the normal literature. This would be more convenient for the researcher, a smaller strain on his memory and pocket, and presumably more economical all round. This is evidently also the view of many who took part in the meetings of the International Association of Meteorology at Rome in 1954, for many of the contributions in the volume under review are given in very brief abstract, or even in title only, with occasional reference to a published paper. Other contributions, one finds, are sometimes rehashes of work already published elsewhere. So the net result can hardly fail to be unsatisfactory (how much simpler life would have been for some of us if one or two classical papers in meteorology had appeared in the normal literature instead of in rare and inaccessible proceedings of the U.G.G.I.).

Even with so many authors summarizing their work, the present volume runs to about 600 pages and this is at least evidence of the intense current activity in meteorological research. The volume has 18 sections, the first given to the presidential address by Prof. K. R. Ramanathan on ' Atmospheric ozone and the general circulation of the atmosphere,' the others to symposia on different branches of meteorology. In case the reader should be surprised to find that meteorology now has so many branches, it would be fair to say that some branches, such as the study of the general circulation and synoptic meteorology, appear under various headings. Even so, the present cover of meteorology is not quite represented in the volume, for there is nothing on convection or small-scale turbulence.

Clearly one cannot discuss the contents of this volume in any detail. It is not, in any case, a book to read through. Sections which have particularly taken the reviewer's eye are those on ozone and stratospheric circulation – 16 contributions, including the presidential address – and on radiation, but there is something for every meteorological taste. The papers on ozone, particularly those by Ramanathan and Paetzold, are of extreme interest in regard to methods of investigation and to the inferences which are beginning to be made about air motions, on all scales, both below and above 20 km altitude. There is a long way yet to go before an adequate description of physical processes in the stratosphere becomes available, but it is already evident that the study of ozone will make a substantial contribution to knowledge of this region.

There is an interesting report of a discussion, led by Dr. Schumann of South Africa, on an international research programme in meteorology. Dr. Schumann pleads for a re-orientation of view in meteorology, for an international effort to cover the southern oceans with a network of observation stations, and for a more highly co-ordinated effort in the publication of data. As is usually the way in such discussions, no firm conclusions were apparently reached but it was doubtless stimulating to those who took part in it and it makes good reading after the event.

The volume is well produced and should be made available to meteorologists through institutional libraries. But it is to be hoped that the publication will not be treated by the International Association of Meteorology as a precedent for future similar productions.

P.A.S.

Handbook of meteorological instruments (Part I) M.O.577. London (H.M.S.O.), 1956. Pp. 458; 132 Figs., 53 Plates; 57 Tables. 45s.

Three years ago the *Quarterly Journal* contained a review of the new Meteorological Office *Observer's Handbook* which dealt strictly with observing routines, leaving the full descriptions of the construction and care of instruments to later volumes. The first of these, Part 1 of the *Handbook of Meteorological Instruments* has now appeared and is a worthy member of what is clearly going to be a useful lineage.

This volume deals with all the usual *surface* instruments used or likely to be used at reporting stations or ordinary observatories. It gives very full instructions for the installation, operation and maintenance of the instruments and also deals with their accuracy, and sources of error. The emphasis is on Meteorological Office practice and on instruments of official design and issue, but where universal (W.M.O.) standards have been adopted they are incorporated. The basic theory of the instruments is given so that the book is quite usable as a moderately advanced text-book. It is authoritative and only a very skilled specialist in his own particular section will be able to find faults, errors or omissions in the book – although it is unfortunate that three years appear to have elapsed between its preparation and appearance.

The English style is a delight – a demonstration of good English, free from either journalese or officialese. The line drawings are good and the printing is clear, but the half-tone illustrations are not quite up to the same high standard.

This book should be on the bookshelf of every meteorologist who uses instruments, alongside the *Observer's Handbook*, and in his bookshelf the reviewer has an adjacent space marked ' reserved for Part 2.'

A.W.B.

Methods of mathematical physics. By Sir Harold Jeffreys and Bertha Swirles (Lady Jeffreys). Cambridge University Press, 3rd Edition, 1956. Pp. ix, 714; Figs.; Tables. 84s.

The first edition of this book was published ten years ago and it must be familiar to many meteorologists; however, the present review may be apposite in view of the mathematical turn that meteorological research has taken in the last decade.

What does an applied mathematician want from a book on mathematical physics ? Unfortunately he changes his mind so often that it must be very difficult for an author to decide what he should include and how he should write about it. Sometimes the applied mathematician wants a list of formulae, such as

Madelung's *Hilfsmittel*, at other times he wants a detailed treatment such as can only be found in large treatises, such as Watson's *Bessel functions*; he always wants his information in a hurry ! To satisfy him completely would need an impossibly large book. Sir Harold and Lady Jeffreys have steered a middle course and written a large treatise containing much that every applied mathematician will want at one time or another.

There is mention of most aspects of mathematics useful to physicists; the authors say that they have tried to include a method which has applications in two or more branches of physics. The first chapter is on the real variable and is probably the one least likely to be used by the physicist; almost all of the chapter is common to any undergraduate course. The next group of chapters covers vector, cartesian tensor and matrix algebra, and is commendably brief in the thorough treatment that is given; indeed economy of statement is one of the most attractive features of this book. Examples of the practical use of the methods developed are given in the text and examples are given at the end of the chapters, usually from some university examination; this illustrative use of practical examples is another most attractive feature of the book.

One of the reasons for writing the book was that Sir Harold's Cambridge tract *Operational methods in mathematical physics* was out of print, so that operational methods are well to the forefront; this requires the introduction of the complex variable and contour integration, sections which will delight readers by their clarity. The whole treatment of operational methods is reminiscent of the tract and is rather spread about in the book in separate sections. Once again the authors are trenchant in their attitude to the use of the Laplace transform as advocated by Carslaw and Jaeger or Van der Pol and Bremmer. The footnote on p. 459 is amusingly exaggerated. The operational method is freely used in the later sections of the book and a table of the interpretations of the principal operators, possibly as an appendix, would have been valuable; this is the sort of result that an applied mathematician wants quickly without having to do the integration. There is no mention of theorems regarding the interpretation when the operator symbol p is replaced by some simple function, such as $p^{\frac{1}{2}}$.

The chapter on numerical methods is a very valuable addition, especially for meteorologists who are so often using such methods on their data. The examples are sufficient to make reference to the larger books unnecessary and there is mention of assorted methods, such as Richardson's method, which are not always noted. The sections on relaxation methods are now much fuller than they were in the first edition. There are also chapters on conformal representation and calculus of variations giving résumés of the parts most needed.

The last half of the book deals mainly with the solution of differential equations and includes some necessary sections on Fourier series and the Gamma function, called here the factorial function for reasons cogently given as a footnote to Chapter 15. The methods of solution of ordinary differential equations are treated in the orthodox manner, followed by a very clear chapter on asymptotic solutions which includes practical examples of the methods of steepest descent and stationary phase.

Chapters 18, 19 and 20 treat the simplest elliptic, parabolic and hyperbolic types of differential equations using especially operational methods as developed in the earlier chapters. There are two chapters on Bessel functions giving the main results, but the reader will still need access to Watson, and one each on the confluent hypergeometric function and Legendre functions; the latter manages to get into a small space what the applied mathematician is likely to need, with the, by now, familiar practical examples. The final chapter deals with elliptic functions, and the appendix on notation should be compulsory reading.

A book of this importance must bear comparison with Courant-Hilbert. It deals with a wider variety of topics than does the older book, and much of the material is not common to both. It is in the solution of differential equations that there is an overlap and here the book under review deals at a less advanced level, for example with only a very slight mention of Green's functions, although it is more modern in outlook. In giving examples from physics in the text and examples to solve at the end of the chapters it has a more practical outlook.

It is scarcely necessary to add that the book is well written and, it would appear, by a single author, for nowhere is a join visible. The well-known Jeffreys chapter-heading quotations are present and the reader can arrange them in order of aptness – the reviewer chose the last one as the best of all. There is a crispness about the writing, which is not without humour, and the clarity we expect from the authors; the compression into some seven hundred pages is a remarkable feat.

The printing and mathematical display are of high standard, and singularly free from errors.

This is a book which anyone dealing in mathematical physics should own – a library copy will not suffice. The price is rather high but represents a capital investment which will pay dividends.

E.K.

Rocket exploration of the upper atmosphere. Edited by R. L. F. Boyd and M. J. Seaton in consultation with H. S. W. Massey, London (Pergamon Press), 1954. Pp. viii, 376; Figs.; Tables. 75s.

This handsomely-produced volume is a special supplement (Vol. 1) to the *Journal of Atmospheric and Terrestrial Physics* and contains 49 papers which were read at a conference held at Oxford in August 1953. The proceedings of the conference, which was jointly arranged by the Upper Atmospheric Rocket Research Panel of the United States and the Gassiot Committee of the Royal Society of London, embraced all aspects of the use of rockets in the direct observation of the properties of the upper atmosphere.

In the early stages of this research, which began eight years ago, the workers were of necessity preoccupied with difficult engineering problems and in designing the instruments and acquiring the special techniques called for in each of the many branches of investigation opened up by the use of rockets as vehicles

for transporting measuring instruments to great heights. The papers comprised in this book on rocket techniques, on measurements of temperature, pressure, winds and the composition of the high atmosphere, and on studies of the ionosphere, solar radiation, geomagnetic variations and cosmic rays, make it clear that the stage has been reached when many of the technical problems have been solved or are near solution, so that it will be possible in the near future to enlarge considerably the already solid achievement.

Besides these papers by leading American rocket workers, there are others by European delegates on related subjects, including radio-echo measurements of meteor ionisation, chemical analysis of upper atmospheric air, ozone determination both by balloon and spectrophotometer, radiation absorption cross sections, and geomagnetic variations. There is a brief account of the French rocket, *Véronique*, at present being tested in the Sahara.

The volume is lavishly illustrated with plates and diagrams.

J.P.

Geochemistry. By the late V. M. Goldschmidt. Edited by Alex Muir, Clarendon Press, 1954. Pp. xi, 730; 9 Figs.; 96 Tables. 63s.

The name of Goldschmidt is so well remembered among geochemists that the publication of a comprehensive work by such a distinguished author is bound to arouse considerable interest. The editor, Dr. Alex Muir, has himself made notable contributions to the subject and, with the help of several colleagues, has produced a book which for many a year will probably be regarded as the standard text-book of geochemistry. Although the major portion was originally written by Goldschmidt himself, the work was not finished when he died. Additional material has, therefore, been added and, in some instances, Goldschmidt's writings have been modified and expanded to bring them up-to-date with recent research.

The book is divided into two parts. The first (125 pages) gives a general account of the subject and deals with such matters as the distribution of the elements, the evolution of magmatic rocks and the chemical composition of the cosmos.

The second and much longer part deals with the geochemistry of the individual elements arranged in groups according to their positions in the periodic table. Three indexes are provided which are comprehensive and include several hundred references to other published works.

The book provides information concerning almost all branches of geochemistry. It is well written and printed and, although parts are translations from the original German, the contents form a well balanced whole. Dr. Muir is to be congratulated on having produced a most valuable and coherent book from what must have been a wide variety of disjointed sources.

To geochemists, Goldschmidt's *Geochemistry* is a book of great importance. Its use, however, will not be limited to specialists, as much of the contents, especially of Part I, are of general interest. The atmospheric physicist and meteorologist, for example, will find many instances of the chemical interaction of the atmosphere, hydrosphere and lithosphere, while the information concerning such fundamental matters as the abundance of the elements, will be of value to both astronomers and physicists.

This book is therefore strongly recommended to all those interested in the composition and evolution of the earth. It also provides a comprehensive and reliable reference book for those concerned with related sciences.

B.C.B.

The generation of electricity by wind power. By E. W. Golding. London (Spon's Electrical-Engineering series), 1956. Pp. xiv, 318, 101 Figs.; 28 Plates; 38 Tables. 50s.

The idea of generating electricity by wind power is fascinating, and a few years ago P. C. Putnam gave an absorbing account of his attempts to do this. He described an enterprise that failed, yet both he and his readers were wiser, and could justifiably feel that the attempt had been worth while. He explained his plan and described the execution of it up to the moment when the tower on which the fan was mounted rocked to its very foundations when one of the blades broke loose and pierced the earth like a dagger some distance away. This was adventure involving the hopes, efforts and ideas of scientists; it was a very human story.

Although Mr. Golding quotes Putman more than any other author he has captured none of his spirit. One has the impression that he has raked through his overloaded filing cabinet to produce nearly 500 references in addition to a 9-page ' selected bibliography of surface-wind data.' Weighed down with such a plethora of authority it is not surprising that the book contributes almost nothing new to the subject. The main problem of how wind flows over hills that are to be chosen as sites for wind power generators remains no nearer solution. We are told that some members of the Electrical Research Association are developing an indefinable know-how about which hills will be good sites (which we were told a few years ago in one of their reports), but that investigations with an electrolytic tank are still in progress so that ' it is therefore not yet possible to say how successful the tank method will be.' The author is therefore somewhat handicapped in his search for a thesis. It is disappointing that he does not philosophise upon why work on this problem has been so unproductive.

However, this is the only book which deals with the subject comprehensively, which is probably the author's real trouble. He discusses the behaviour of wind over hills, the problem of wind measurement,

the design of wind machines, the economics of the use of wind power and its relation to other sources – but none with obvious expertness, each being a topic enough for a whole book. If there has been no dramatic success, this book makes plain the nature of the problems and the limitations of present ideas, particularly on the meteorological side, that have prevented it; and it is to be hoped that the challenge will be taken up.

The central problem of turbulent wind flow over humps is very difficult : it may seem rather special, but it offers scope for the greatest insight and philosophical skill. The problem of using power in a form in which it can easily be extracted from the wind is one to tax engineers of the highest ingenuity, and to read Mr. Golding's book makes one appreciate this.

It is a pity that he has not chosen a more narrative form in order to spotlight what has already been achieved. There is a story to tell and bits of it are there; but if it had been emphasized more, at the expense of the ' woulds ' and ' coulds ' and ' mays,' the feeling of loss of direction at the end of the book would have been avoided.

The indexes are awful. Presumably the purpose of an index is to enable enquirers to find information about the subjects listed in it. In the subject index there are far too many entries and the information to be found is generally either non-existent or trivial. The name index, containing about 300 names is quite pointless, and often leads only to one of the references at the end of a chapter. In many cases the reference is only mentioned, not discussed, in the chapter, and sometimes not even mentioned (thank goodness).

The making of a good index is the author's tedious but worthwhile responsibility, and in a many-sided book is very desirable. If there is to be a second edition here is ample scope for improvement.

R.S.S.

Weather and the land. Bulletin No. 165 of the Ministry of Agriculture, Fisheries and Food. London (H.M.S.O.), 1955. Pp. iv, 35; 14 Figs.; 4 Plates; 3 Tables. 3s.

This small book, produced to give farmers and growers a better understanding of the facts lying behind the official weather forecasts, contains much interesting material. It discusses, among other subjects, the length of the growing season in different parts of the British Isles, precautions against frost, and the right length and thickness for shelter belts of trees. Two recent advances, the forecasting of outbreaks of potato blight, and the calculation of the need for irrigation, are briefly described. Farmers will be interested in these sections of the book, and they will find the last chapter, which gives details of the available meteorological services, very useful.

It is a little doubtful, however, whether they will get as much benefit as they ought from the first and fourth chapters, which give an outline of meteorology, intended to enable the reader to understand, and if necessary to amend, the official forecasts. Unfortunately neither chapter is as clearly written as it should be, mainly because the language used is too technical. The author does not seem to have borne in mind that he was writing for farmers and not for his fellow meteorologists, for he uses words like ' precipitation,' etc. Together with an excessive use of technical terms goes a failure to make it clear that depressions, fronts and anticyclones are not merely lines on a chart; perhaps this could have been avoided if the two chapters had been combined, so that the introduction of each term such as ' depression ' (which is given in chapter I) was followed immediately by a full description of the weather to be expected in that particular situation (given in chapter 4). As it is, a great deal of turning pages back and forth will be needed to get the best out of these two chapters.

J.M.

Realms of water – some aspects of its cycle in nature. By P. H. Kuenen. London (Cleaver-Hume Press). Revised version translated by May Hollander, 1955. Pp. 327; 190 Figs.; 16 Plates; Tables. 35s.

The scope of this book covers not merely the cycle of water in nature from the purely hydrological standpoint but it includes practically every conceivable aspect of water from its chemical composition to the part it has played in carving out the landscape through the ages; in fact, it brings into account nearly every effect water has upon the physical environment both in its liquid and in its solid form.

The wide range covered makes it of particular value to the physical geographer and it undoubtedly will become a standard text-book in the shelves of geographical libraries. More space could possibly have been devoted to the part played by plants and vegetational cover; the meteorological portion, too, in the chapter on water in the atmosphere is somewhat ' thin,' if not in certain cases misleading. But the author is a geologist and primarily his interests are more likely to lie in the realms of sedimentation – marine and terrestrial – and it is in his chapters on the oceans, the coast, the landforms and sub-surface waters that he is at his best.

The book is illustrated throughout by an abundance of excellent line-block diagrams and photographs. As random examples of their worth, one series depicts the successive stages in the formation of a stalactitic pillar, a second shows sliding meanders in a river system formed when vertical and lateral erosion takes place simultaneously and a third shows a series of melt holes in a glacier. Most diagrams are original products often in three dimensional block form; some are borrowed from other authors. These illustrations, together with a simply and lightly written text, make this book a very valuable contribution to the physical activities of water on the earth's surface, particularly in respect of sedimentation.

C.D.O.

Soil physics. By L. D. Baver. New York (John Wiley), London (Chapman and Hall), 3rd Edition, 1956. Pp. xvii, 489; 106 Figs.; 65 Tables. 62s.

The issue of a third edition of ' Baver ' is an indication that the only available book of its kind is successfully satisfying a real need. The need is that of ' advanced undergraduate students of fairly good calibre and graduates ' (author), in agronomy or kindred subjects (reviewer). The physicist who attempts to read it will be disconcerted by the many technical statements he has to accept without any explanation; dismayed by faulty physics in definitions or statements (e.g., ' The magnitude of the charge is known as the zeta potential '); and puzzled to understand why a book on the physics of the soil should not be at least as stimulating as several of the available texts on the physics of the atmosphere.

The meteorologist has less reason to find anything of interest. Weather topics occur in places, and include effect of frost on soil structure, effect of rain on soil structure and erosion, gas and vapour exchanges between soil and atmosphere, evaporation from bare and cropped soil, and meteorological aspects of irrigation control.

Although there are some gaps, the bibliography is extremely good and wide-ranging and, apart from anything else, this will ensure that the third edition maintains the reputation of the first two as an essential book in any library of soil science.

H.L.P.

Wind-tunnel testing. By Alan Pope. New York (John Wiley), London (Chapman and Hall), 2nd Edition, 1954. Pp. xv, 511; 337 Figs.; 24 Tables. 68s.

Meteorologists normally use wind tunnels when they want to reproduce some condition of airflow associated with the wind or when they are interested in calibrating wind-measuring instruments. Only the few specialists who design and use instruments to be taken up in aircraft need to use the sort of wind tunnels and deal with the problems which Mr. Pope describes. The author is a practising wind-tunnel engineer and teacher in the subject and his book at times gives one the impression of being a selection of workshop notes or parts of a lecture jotted down. An improvement in clarity could have been obtained had the author taken greater care with the logical presentation of his facts; this does not detract from the value of the advice he gives but it does make difficulties when one wants to find a particular piece of information.

Of the 12 chapters, the first three deal with the design of the tunnels themselves and of the instruments and gauges used in the tunnels, the next two chapters cover the subject of models and the best ways of extracting the information required by the aircraft designer. These are followed by two chapters about the manipulation and reduction of the data and include a comprehensive account of all the various wall corrections which must be applied if the data are to be representative. This chapter on corrections, I found, suffered most from the author's casual style and one is obliged to read right through the text to extract the formulae one needs. However as the author states in the preface, this is a book for training students working in the subject, and therefore it can perhaps be excused from being as well a handbook for those whose main interest lies outside wind tunnels. The training aspect of the book is emphasized by the inclusion of many examples, and worked examples are frequently inserted.

There are two small chapters, of 13 pages in all, on the topic which meteorologists might be expected to turn to first, namely non-aeronautical uses of wind tunnels, and small tunnels. The subject however is treated purely from the point of view of the engineer concerned with the strength of a structure in the wind, or the avoidance of pollution due to fumes or smoke.

The last three chapters deal with rotors, near-sonic, trans-sonic and supersonic testing.

Experimental meteorologists sometimes borrow surprising tools from other sciences. It might well happen therefore that the aerodynamicists in the meteorological camp may want to read about wind-tunnel balances, and, of course, anyone making quantitative measurements in a wind tunnel will want the chapter on wall corrections. Mr. Pope's book is, however, really intended for wind-tunnel engineers engaged in testing models for aircraft designers on a commercial basis.

D.D.C.

The sun and its influence. By M. A. Ellison. London (Routledge and Kegan Paul), 1955. Pp. xii, 235; 54 Figs.; 9 Plates; 9 Tables. 21s.

Meteorologists may expect to find some consideration of the question of the possible influence of solar energy variations on weather in a book with this title but they will not cavil with the author for eschewing this highly controversial topic, especially since there has become available in recent years such a mass of factual information, garnered in the various other fields of research in solar-terrestrial relations. The task of assembling in a logical, accurate and readable form the continuously accumulating results of research in widely different subjects often related to each other only in the sense that the phenomena which are their concern owe their origin to events in the sun, is formidable and it must inevitably involve the simplification which may be dangerous when applied to a rapidly developing subject. It can be successfully accomplished only by one who, besides being actively engaged, and therefore well versed, in the fields of research considered, also possesses the skill in presentation which usually derives from wide experience in teaching. That Dr. Ellison is well endowed with these qualities will become abundantly clear to every reader of this excellent book. It may be read with profit and pleasure not only by the general reader with an elementary knowledge of physics but also by the specialist in related fields.

After several introductory chapters dealing with the constitution, sources of energy, surface activity and 'atmosphere' of the sun, the various terrestrial phenomena – ionospheric, magnetic and auroral – which result from solar activity are examined in turn. Finally an account is given of the emission of radio waves and of cosmic rays by the sun.

The book is well produced and profusely illustrated by good line diagrams and photographs.

J.P.

Equatorial weather : with particular reference to south-east Asia. By I. E. M. Watts. University of London Press, 1955. Pp. xvi, 224; 129 Figs.; 8 Plates; 10 Tables. 25s.

Though this is in many ways an excellent little text-book it is not altogether satisfying. The reasons for this opinion are not easy to state precisely, but they arise from uncertainty, which prolonged consideration has failed to resolve, as to the class of reader for whom the book is really intended. It is stated in the preface that the book seeks to serve needs in colleges and to provide aviators and seamen with guidance on weather in low latitudes. It assumes some acquaintance with the elements of synoptic meteorology and the physical processes of weather. The doubts, in the mind of the present reviewer, as to the real purpose of the book arise from an uneven treatment of the subject; this ranges from a most elementary explanation of the vertical variation of temperature in the troposphere to well-known theoretical but unrealistic discussions of air movement across the equator, and the numerical estimation of horizontal convergence from observations of wind. The author refers freely to the work of others in tropical meteorology, but in the field of analysis, at least, his selection is undiscriminating and uncritical.

The book contains fifteen chapters which it appears may be divided into three principal sections, though this is not immediately evident and the order of treatment is perhaps not the best. The first of these sections, consisting of Chapters I, III, IV and V deals with those principles which would be included in any general text-book, namely, general circulation, lapse-rate, the tephigram and convection, formation of fog, cloud and precipitation; the treatment has special reference to tropical regions but is very elementary, and some of the explanations are somewhat naïve. Chapter II is a digression for the discussion of the interpretation of meteorological observations in low latitudes having regard to such factors as diurnal variation of the elements and the breakdown of the geostrophic wind relation.

The block of Chapters VII to X deals with equatorial air movements, convergence and divergence, air-mass boundaries and the associated cloud formations. This is the least satisfying part of the book; it will confuse the beginner and will carry little conviction for the experienced synoptician. However, emphasis is correctly laid on mapping the field of air movement rather than the pressure field as a principal synoptic technique, and some instructive examples of air-stream analysis are given in Chapter XII.

The best parts of the book are the more descriptive ones dealing with rainfall types, air circulations and local disturbances of south-east Asia; they, however, are interspersed with the other chapters. They are informative and well illustrated by many line diagrams.

Appendices deal with synoptic codes and plotting symbols fully enough to be regarded as a set of instructions for the plotting of charts; in the reviewer's opinion a shorter list of symbols and codes for the purpose of interpreting the specimen synoptic charts would have sufficed in a book of this kind.

A.G.F.

Les applications du radar à l'astronomie et à la météorologie. By J. van Bladel. Paris (Gauthier-Villars), 1955. Pp. vi, 147; 55 Figs.; 6 Tables. 1,600 fr.

The newcomer to radar meteorology or radar astronomy must seek information in numerous journals devoted to physics, electronics, geophysics, meteorology and astronomy, and in reports with limited circulation; he will be grateful for Dr. van Bladel's book, which covers the field adequately and refers to nearly 200 papers written between 1945 and 1954. The main emphasis is on meteors and on precipitating clouds, which receive 40 pages each.

Chapter 1 is a brief account of radar principles, omitting circuits. The definition of bandwidth is needlessly unconventional, while Fig. 3 (b) suggests that radio receivers have zero gain at the frequency to which they are tuned! Here – and later – it is erroneously stated that isotropic targets do not reflect circularly polarised waves.

Chapter 2 outlines the requirements for inter-planetary radars, but is rather optimistic about the improved measurements of solar parallax that radar can give. The present accuracy of the astronomical measurements is 1 in 5000, not, as stated, 1 in 1000.

Chapter 3 is a full discussion of meteor echoes, including the measurement of velocities and orbits, and the determination of the total meteoric flux falling on the earth. Here, as throughout the book, explanations are often left to equations, with too little discussion. Thus the equation for the changing echo intensity from a moving meteor is given, without pointing out the useful analogy with diffraction of a cylindrical wavefront at a straight edge (Cornu spiral). The symbol J is wrongly defined as the diffusion coefficient, not of electrons in a neutral plasma in air, but of electrons alone in air – the two coefficients differ by a factor of 1000. The account of the decay of meteoric ionization omits the important differences in the process by which *atomic* meteoric ions and *molecular* atmospheric ions recombine. A good account of the concept of the meteor echo plane is marred by the description of Clegg's method of determining radiants.

Chapter 4 is an already obsolete and uncritical account of auroral echoes, and includes a short section on lightning echoes which fails to mention Hewitt's work.

Chapter 5 is a full account of precipitation echoes, which includes short sections on echoes from tropospheric discontinuities and from non-precipitating clouds. There is a full and up-to-date account of the scattering properties of precipitation elements, though the discrepancy between Ryde's theory and many measurements of echo intensity is dismissed too easily. The intensity fluctuations of rain echoes are very badly explained – the equation for the first probability distribution of intensity is left to explain itself, as is the startling remark that ' the instantaneous echo power can assume any value.' An excellent account of radar pluviometry, a subject rather neglected in this country, is most welcome.

There are a fair number of minor errors and misprints; diagrams are often very inadequately explained, and the quality of most of the half-tone diagrams is poor. The printing of the mathematics could be much improved. The system of references is not good and there is no index, while however useful the separate list of symbols, it is no substitute for repeating definitions in the text whenever symbols recur after several pages of disuse.

I.C.B.

The Planet Venus. By Patrick Moore. London (Faber and Faber), 1956. Pp. 132; 13 Figs.; 8 Plates; Tables. 15s.

This book is in the same series as de Vaucouleur's *Physics of the Planet Mars* but it differs very greatly from its predecessor. The material upon which it is based is much thinner and it is written in a frankly popular style, from the point of view of the visual observer rather than the astrophysicist. There is a very great variation in the quality of the book on different topics. On visual observations it is interesting and convincing; on physical measurements it is inadequate and rather naïve; on speculative theories the exposition is far too brief for credibility.

The meteorologist will mainly be interested in physical measurements and arguments concerned with the atmosphere, and he is unlikely to be satisfied by what he reads. For example, reports that the unlit segment of Venus can be seen *dark* against the daylight sky are said to be due to optical illusion but the author thinks it is ' impossible to be sure.' Many of us would not be so hesitant. On the other hand, on the composition of the atmosphere, we learn that ' Carbon dioxide is a heavy gas, and would normally be expected to sink rather than rise. This indicates a more or less homogeneous composition all the way down, but we cannot be sure.' This time the author has cause to hesitate.

On visual observations, however, the author writes well and with confidence and this book will probably commend itself to the amateur astronomer.

R.M.G.

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THE NAPIER SHAW MEMORIAL PRIZE

Announcement of second competition

The Royal Meteorological Society announces the second competition for the Napier Shaw Prize. The competition will be open to anyone without restriction of nationality. The prize of £100 is offered on this occasion for an original essay on **'Climatic variation.'**

> The essay may deal with climatic variation on any time scale or scales. A critical analysis and appraisal of the evidence is sought concerning a large part of the earth's surface and preferably on a global basis. The use, where suitable, of modern statistical theory in the interpretation of the evidence will be welcome. Theoretical explanation in geophysical terms may also be offered.

The essay, which must be in English, may include material which the candidate has already published. Essays may be submitted jointly in the names of more than one author.

Candidates are required to submit three copies of their essay, which must be printed or typewritten. In the latter case, only one side of the paper should be used, double spacing should be employed and two of the copies may be good carbon copies. Unless the essay is printed, any mathematics should be written in carefully by hand. Although the scale of the essay will be considerably greater than that of an ordinary paper, candidates are recommended to consult, for general advice, the pamphlet, 'Preparation of papers for the Quarterly Journal.' This may be obtained, free of charge, on application to the Assistant Secretary.

It is required that any essay for which a prize is awarded shall be offered to the Society for publication.

All entries for the competition should be addressed to the Assistant Secretary, Royal Meteorological Society, 49 Cromwell Road, London, S.W.7 and marked clearly 'Napier Shaw Prize.'

Essays for the competition must be received at the Society's Offices *not later* than 1 January 1959.

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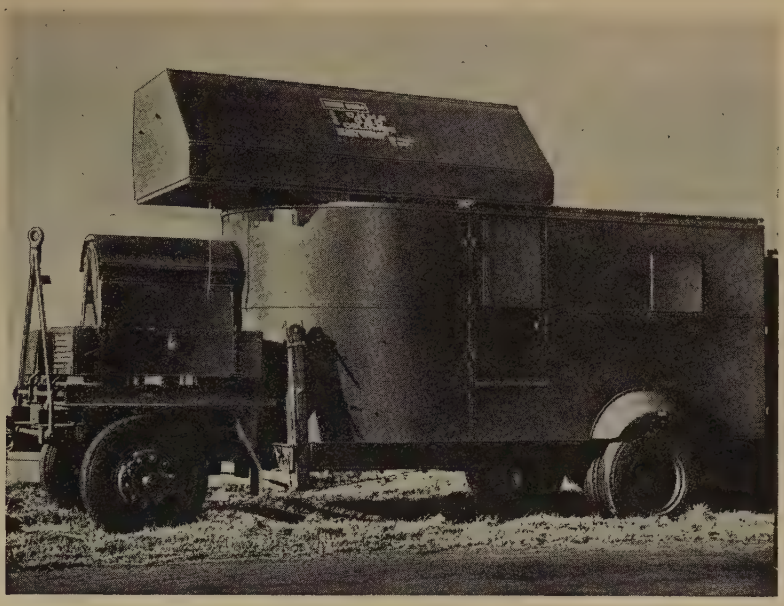

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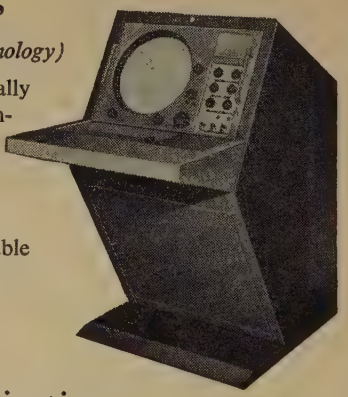

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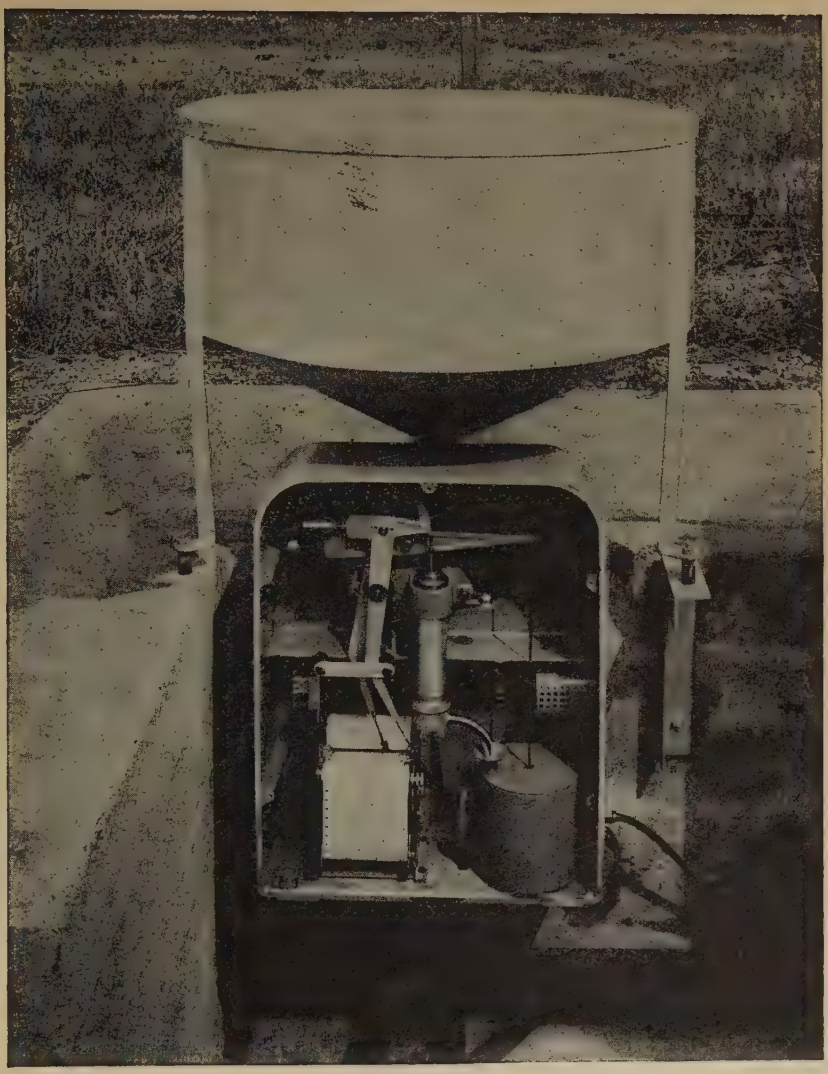

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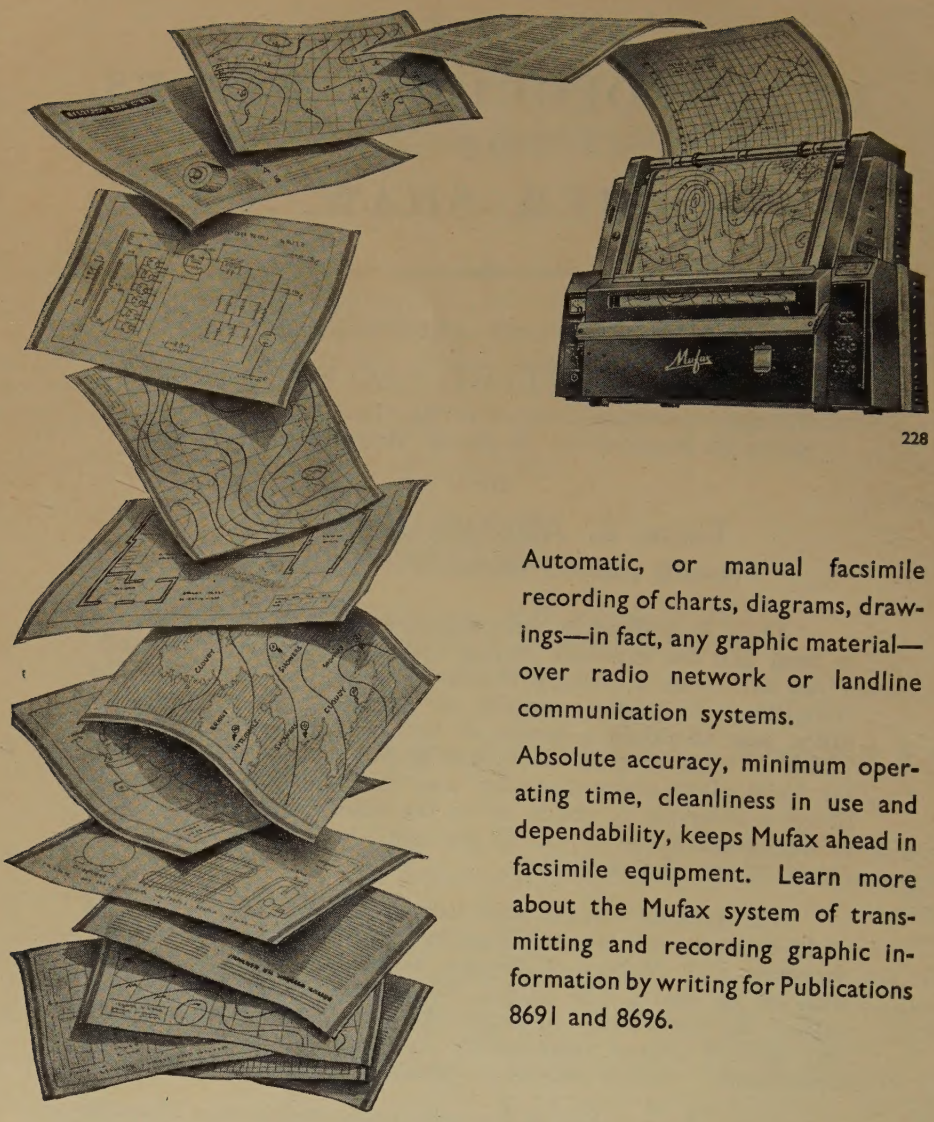

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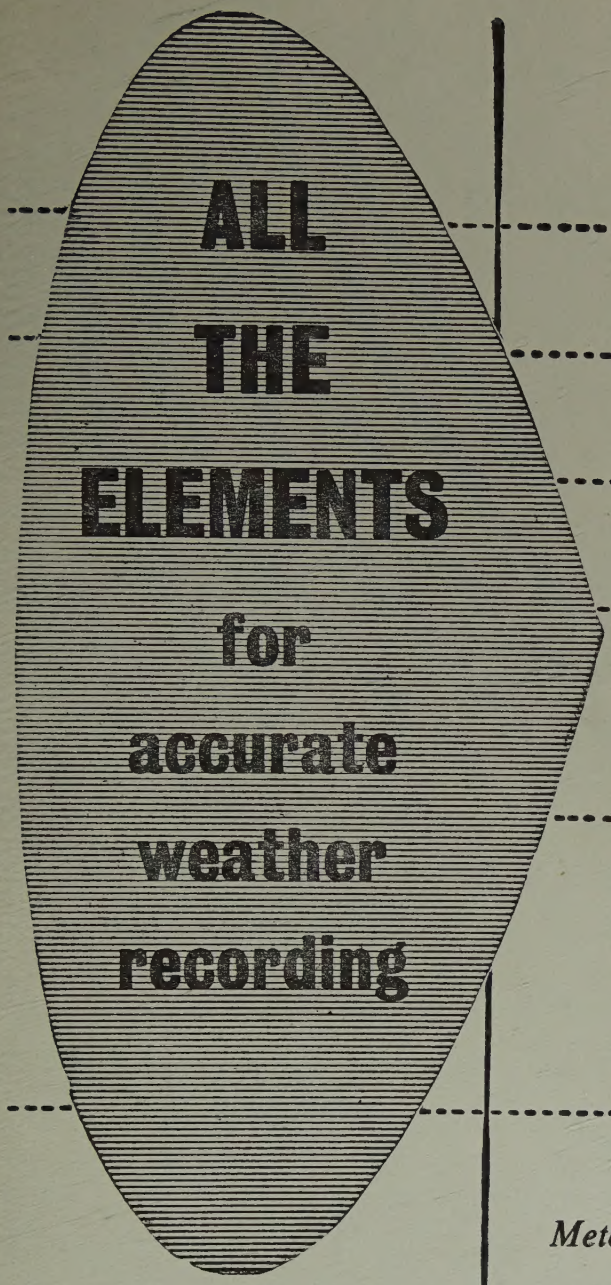

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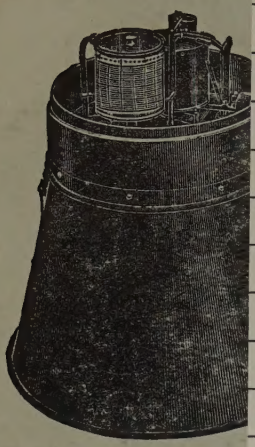

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